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THE WORKS OF HENRY J. S. SMITH.

The Collected Mathematical Papers of Henry John Stephen Smith. 2 vols. Edited by Dr. J. W. L. Glaisher. (Oxford: Clarendon Press, 1894.)

THE long looked for collected papers of Prof. H. J. S. Smith, late Savilian Professor of Geometry in the University of Oxford, have now appeared in two handsome quarto volumes issued by the Clarendon Press at Oxford. This fact is, as far as England is concerned, the mathematical event of the year, and is of the utmost importance to mathematicians in general, and to the rising race of investigators in pure mathematics in particular. The work has a portrait on the frontispiece, and is introduced by a biographical sketch by Dr. Charles H. Pearson, and recollections by Prof. Jowett, Lord Bowen, Mr. J. L. Strachan-Davidson, and Mr. Alfred Robinson, also by an introduction by Dr. J. W. L. Glaisher. A perusal of the sketch is calculated to greatly impress the reader with the all-round scholarship and intellectual eminence of its subject. To have gained, amongst other honours, the Ireland University Scholarship, and subsequently to have become one of the most profound and rigorously exact mathematicians the world has ever known, implies the possession of powers of mind that must fill any chronicler or student of past events with amazement. There are men who will succeed in any line of life or branch of study by sheer mental strength; they have the faculty of becoming fascinated by any pursuit in which inclination, force of circumstances, or accident leads them to engage; with them study is intense concentration leading, through a flood of new ideas, to such an admiration for and interest in the subject, of whatever kind, as can only be experienced by another in some special branch for which his mind is particularly and peculiarly adapted.

That Prof. Smith was such a man, was the general belief of his contemporaries. Prof. Conington said to the biographer, "I do not know what Henry Smith may be at the subjects of which he professes to know something; but I never go to him about a matter of scholarship, in a line where he professes to know nothing, without learning more from him than I can get from anyone else."

At one time it appeared to be probable that he would devote himself to chemical science, but, looking back, there seems to be little doubt that pure mathematics was the branch of knowledge towards which he felt himself most attracted, and which was in reality best adapted to call forth his grand powers for close and accurate thinking, and to give scope to his brilliant imagination.

The recollections are of great interest. They show clearly the extent to which he was admired and loved by those who were privileged to know him best. Dr. Glaisher's introduction is chiefly, though not wholly, of mathematical interest, and will be further alluded to. The works set forth were published between the years 1851 and 1883. They may be considered as arranging themselves under four heads: (1) Theory of numbers; (2) elliptic functions; (3) geometry; and (4) addresses. Space merely permits me to note some of the original contributions to science which stand forth pre-eminently, and helped to build up a great reputation.

During the years 1859-1865 was produced the "Report on the Theory of Numbers," compiled for the British Association for the Advancement of Science. Prof. Smith said of Clifford that he was "above all and before all a geometer"; so of him it may be said that he was above all and before all an arithmetician, and that the Report could not have come under a stronger hand. It contains an account, confessedly not exhaustive, of the state of knowledge at the date of writing. There is interpolated in the history of the science, as it was originated by Gauss and Legendre, and developed by Cauchy, Jacobi, Lejeune-Dirichlet, Eisenstein, Poinsot, Kummer, Kronecker, and Hermite, a considerable amount of masterly criticism as well as original work. He considers the higher arithmetic to be comprised of two principal branches, the theory of congruencies and the theory of homogeneous forms. It will be observed that he does not include the combinational or partitional analysis. He doubtless did not regard this important subject as a branch of arithmetic proper, but rather as occupying the ground intermediate to arithmetic and algebra. It is, in point of fact, far less abstruse and less dependent upon methods which are regarded as purely arithmetical. In the future, however, it is probable that it will be recognised that the combinational analysis is able to throw quite unexpected light on the theory of congruencies, and is worthy of being considered as an important instrument of research in arithmetic proper. As an example it may be stated that the enumeration of certain permutations on a circle yields the number

$$\frac{1}{n} \Sigma \phi (d) \, x^{\frac{n}{d}},$$

where x and n are any positive integers, d a division of n, and $\phi (d)$ the totient of n; and hence

$$\Sigma \phi (d) \, x^{\frac{n}{d}} \equiv o \bmod n$$

a congruence which includes several of the elementary results of the theory of numbers. The author's intention was to present the theory of homogeneous forms in the following order:—(1) Binary quadratic forms; (2) binary cubic forms; (3) other binary forms; (4) ternary forms; (5) other quadratic forms; (6) forms of order n decomposable into n linear factors. It is much to be regretted that only the first of these was given in the report. A consideration of the remaining divisions seems to have convinced him that much remained to be done, and he appears to have deferred these matters for future investigation by himself. The solution of the so-called "Pellian Equation" is of primary importance in the theory of quadratic forms of positive and not square determinant, and we find in the foot-note of p. 193, vol. i. "There does not seem to be any ground for attributing either the problem or its solution to Pell." This is particularly interesting to those who were privileged to listen to Prof. Mittag Leffler's paper on automorphic functions, which was read before Section A of the British Association at Oxford. The Professor inveighed against the too common practice of associating mathematicians' names with theories and theorems on the ground that mistakes are of frequent occurrence and necessarily so;

he instanced the use made by Poincaré of the names of Fuchs and Klein in regard to theories, priority in respect of which those eminent men would be the first to repudiate. In the present instance we find that the problem of the "Pellian Equation" was proposed by Fermat and solved by Lord Bronncker, and these facts need not detract in the least from the reputation earned by Pell by his skill in the Diophantine analysis.

In the solution of the problem of the "Composition of Quadratic Forms," Prof. Smith introduces the important notion of fundamental sets of solutions of indeterminate systems of equations, and thus replaces Gauss' purely synthetical solution by analysis. In the arrangement of the genera of quadratic forms into classes he extends to irregular determinants the principles employed by Gauss for the case of regular determinants.

Gauss' geometrical representation of forms of a negative determinant is given at length. Klein has recently, in the lectures on mathematics delivered before the Evanston Colloquium in the autumn of 1893, given a remarkably simple statement of the method, and has introduced the expressions "line lattice" and "point lattice" to describe the diagrams. He also has extended the method to forms of positive determinant in the *Göttingen Nachrichten* for January 1893. To this the reader's attention may be directed as elucidating and amplifying Prof. Smith's statement of the work of Gauss. Klein's lecture VIII. (Evanston) should also be referred to in connection with the theory of complex primes and the ideal numbers of Kummer.

On the completion of the report, his attention was directed to the subject of ternary quadratic forms. At the time an important memoir by Eisenstein had appeared, in which were defined the ordinal and generic characters of ternary quadratic forms of uneven determinant; but several of the results were left undemonstrated. Prof. Smith supplied the omissions, and extended the results to the more difficult and complicated case of the even determinant. By giving a table for forming the complete generic character of any form, he accomplished for the ternary theory that which had been already carried out by Lejeune-Dirichlet for the binary theory. He gave, moreover, a demonstration of the criterion for distinguishing between possible and impossible generic characters. This he was enabled to do by the important new notion of a certain particular generic character, termed by him "the simultaneous character of a form and its contravariant," which had not been regarded by Eisenstein. He gave a more complete definition of a "genus" of forms as dependent upon transformation by substitutions, and showed that two forms are or are not transformable into one another according as their complete generic characters do or do not coincide. He proved the formulæ which assign the weight of a given genus or order both for even and uneven discriminants. This he accomplished by a comparison of two expressions, obtained by different methods, for the limiting ratio of the sum of the weights of the representations, by a system of forms representing the classes of any proposed genus, of all the numbers contained in certain arithmetical progressions and not surpassing a given number, to the sesquiplicate power of the given number when that number is indefinitely large. This paper is one of great power, constituting one of his most important contributions to science.

The above was followed by another great work "On the Orders and Genera of Quadratic Forms" containing more than three indeterminates. This paper will always be a celebrated one in the history of mathematics. It contains under date 1867, implicitly, the solution of the problem proposed fifteen years later for the Grand Prix des Sciences Mathématiques by the French Academy. The problem referred to was given as "Théorie de la décomposition des nombres entiers en une somme de cinq carrés." In the paper of 1867 it was indicated that the four, six, and eight-square theorems of Jacobi, Eisenstein and Liouville were deducible from the principles set forth. He then completed Eisenstein's "enunciation" of the five-square theorem by bringing under view the numbers which contain a square divisor, and added the corresponding seven-square theorem. The demonstrations were not given, but a general theory, which includes these theorems as corollaries, was given in detail. On these facts being pointed out to Hermite, a correspondence ensued, which the reader will find given, with comments, in the introduction by Dr. Glaisher. The result was that Prof. Smith sent in his demonstrations, and that ultimately the prize was divided between him and M. Hermann Minkowski, of Königsberg. The latter memoir followed closely the lines of the paper of 1867, a fact which gave rise at the time to much discussion concerning the action that was taken by the French Academy. The prize memoir is the concluding paper of vol. ii.

Passing over, for want of space, other arithmetical work of much value, a few words may be said concerning the papers on elliptic functions, which constitute the bulk of the second volume.

The paper, "Mémoire sur les équations modulaires," contains a theory of singular beauty. Mathematicians were aware, thanks to profound researches of Kronecker and Hermite, of the intimate relations that exist between the theory of binary quadratic forms of negative determinant and the transformation of elliptic functions, but beyond Kronecker's elliptic function solution of the "Pellian Equation," no association had been discovered between the binary quadratic forms of positive determinant and the elliptic functions.

In this paper it is shown that if

$$F(k^2, l^2) = 0$$

be the modular equation for the transformation of order N, the Cartesian equation

$$F(\tfrac{1}{2} + X + iY, \tfrac{1}{2} + X - iY) = 0$$

is a curve which gives an exact image of the complete system of forms of positive determinant N. By the simple process of enumerating the spirals and the convolutions of each spiral, he determines the number of non-equivalent classes and the complete system of "reduced" forms in each class.

In "Notes on the Theory of Elliptic Transformation" will be found a complete discussion of the case in which the modular equation has equal roots; it is shown that the squares of the corresponding multipliers are always different, and that this is consistent with Kœnigsberger's

theorem, which states that the multiplier is a rational function of the squares of the moduli. The latter is shown, in fact, to break down when the modular equation has equal roots.

The long memoir on the Theta and Omega Functions was originally written as an introduction to the long-expected "Tables of the Θ Functions." It may be regarded as an advanced work on elliptic functions, in which the arithmetical treatment is given the prominent place. The theory of the transformation, and in particular of the modular equations and the associated curves, is exhibited with remarkable elegance.

Everywhere the treatment is characterised by extreme rigour. In fact, the subject matter, dealt with in these volumes, leads to work of so recondite a nature that only an investigator to whom any slurring over of difficulty, or exceptional case is absolutely repulsive, can expect to make a real advance. Those who look chiefly to results, and do not care to know the precise circumstances under which they exist, may be warned off the monument to Prof. Smith's genius which is given to the world in these pages.

On two principal occasions Prof. Smith found opportunity to place his views on mathematics in general before the scientific world. We have the valedictory address to the London Mathematical Society, delivered in the year 1876, on his retiring from the office of president. He took as his text some "comparatively neglected regions of pure mathematics"; and now, after an interval of eighteen years, it is a matter of great interest to re-survey the ground and estimate the advances that have been made. In the theory of numbers, then as always the subject of his predilection, he called attention to the state of knowledge with respect to (1) the theory of homogeneous forms; (2) the theory of congruences; (3) the determination of the mean or asymptotic values of arithmetical functions. With respect to quadratic forms of four or more indeterminates, he referred to the fundamental theorem of M. Hermite concerning the finiteness of the number of non-equivalent classes of forms having integral coefficients and a given discriminant; and to the researches of Zolokoreff and Korkine on the minima of positive quadratic forms. In a foot-note also he referred to his own great work "On the Orders and Genera of Quadratic Forms containing more than Three Indeterminates." These three papers mark the extent to which the inquiry had been pushed at that time. The latter is much the most important, and, so far as I know, but little further progress in the same direction has since been made. In the theory of congruences an important advance has been made by G. T. Bennett, in a paper published in the *Phil. Trans. R.S.* vol. 184A. The investigation is "On the Residues of Powers of Numbers for any Composite Modulus, Real or Complex." Remarking that primitive roots exist only when the modulus is a power of an uneven prime or the double of a power of an uneven prime, and that a primitive root may be said to "generate" by its powers the complete set of residues, Bennett exhibits the mode of formation, and the relations connecting, the most general set of numbers capable of generating the $\phi(m)$ numbers which are prime to any composite modulus m, and extends his results to complex numbers.

Prof. Smith gave an historical account of our knowledge of the series of prime numbers. Prof. Sylvester has made a considerable contraction of Tchébychef's limits, and has established important general principles in connection therewith.

Passing on to the discussion of the transcendency of e and π, it may be noted that since the address was delivered (in fact, six years subsequently) the question has been triumphantly set at rest for ever by the labours of Hermite and Lindemann. The former established the transcendency of e, and the latter, standing on the shoulders of the former, demonstrated the transcendency of π. Lindemann's proof shows that π cannot be the root of any algebraic equation, and marks a distinct epoch in the history of mathematical science. The death-blow was thus given to the circle squarers in 1882 (*Math. Ann.* vol. xx.). Quite recently extraordinarily simple proofs of the transcendency of both numbers have been given by Hilbert. Prof. Smith noted and lamented the want of advanced treatises in English on various branches of pure mathematics. Our position to-day in this respect exhibits a marked improvement. On differential equations, theory of functions, integral calculus, theory of numbers, important works by English and American authors have been published, and certain eminent mathematicians are known to be engaged in the preparation of advanced works, which will shortly appear and further fill in the gaps.

Prior to the above, in 1873, was delivered the address to the Mathematical and Physical Section of the British Association. Remarking on the recent appearance of Maxwell's "Electricity," he observes: "It must be considered fortunate for the mathematicians that such a vast field of research in the application of mathematics to physical inquiries should be thrown open to them at the very time when the scientific interest in the old mathematical astronomy has for the moment flagged, and when the very name of physical astronomy, so long appropriated to the mathematical development of the theory of gravitation, appears likely to be handed over to that wonderful series of discoveries which have already taught us so much concerning the physical constitution of the heavenly bodies themselves." Mathematical astronomy to-day, it may be said, no longer flags. Thanks to the work of Hill, Poincaré, and Gylden, the subject has received a new impulse, and the world of science watches with intense interest the process of its evolution under the powerful hands of these mathematicians.

Prof. Smith had much at heart the organisation of scientific education as influencing the supply of scientific men. He asserts the importance of assigning to physics a very prominent place in education. He gives as his opinion that from the sciences of observation the student "gets that education of the senses which is after all so important, and which a purely grammatical and literary education so wholly fails to give." These are weighty words when we consider the all-round attainments of their author, and that he was, in particular, a classical scholar of the first rank. The effect of these volumes on

the progress of research is sure to be considerable. A student will have before him work whose style has never been surpassed, and demonstrations which are absolutely rigorous. In the latter respect Gauss' work seems to have left a lasting impression upon his mind.

I conclude by quoting the noble words from the British Association address :—

"But in science sophistry is impossible; science knows no love of paradox; science has no skill to make the worse appear the better reason; science visits with a not long-deferred exposure all our fondness for preconceived opinions, all our partiality for views that we have ourselves maintained, and thus teaches the two best lessons that can well be taught—on the one hand the love of truth, and on the other sobriety and watchfulness in the use of the understanding."

P. A. MACMAHON.

ABSTRACT GEOMETRY.

Grundzüge der Geometrie von mehreren Dimensionen und mehreren Arten gradliniger Einheiten in elementarer Form entwickelt. Von Guiseppe Veronese. (Leipzig: Teubner, 1894.)

MODERN speculations on the Foundations of Geometry have raised the question of the character of Geometry as a science, and the question has been answered in different ways. Some writers have held that our space-intuition is an absolute guarantee of the truth of geometrical axioms; others have treated Geometry as a science of observation and experience, whose results accordingly are liable to the same kind and degree of inexactness as any other Physical Science. If either of these answers were correct, the method of Geometry would seem to require revision. The method is to deduce the properties of figures by logical processes from definitions and a few propositions (Axioms) assumed in advance. But if space-intuition were a sufficient guarantee for the truth of the Axioms, it would seem to serve equally well for a guarantee of the truth of many of the Propositions, and there would appear to be no good reason for assuming as few as possible and deducing the rest. If, again, Geometry is to be purely a Natural Science, there would be simplicity in proving its propositions by the help of well-made constructions and good instruments of measurement. There seems to be room for a third view of Geometry as an abstract formal science to which the method always known as geometrical would be proper and natural. In such a view abstraction might be made of all space-intuition, and there would remain a body of logical truths in which the Axioms would occupy the place of Definitions or well-defined Hypotheses. The science would be at the same time founded upon intuition and independent of intuition. If its Definitions and Hypotheses are never in contradiction with themselves, or with each other, or with our space-intuition, then will its conclusions always be verified within the limits of exactness that belong to observation. It will be a formal science ready for practical applications.

The theory of Abstract Geometry in the sense just described is the subject-matter of Prof. Veronese's treatise. He lays down in his Preface the nature of Geometrical Axioms as the simplest truths of space-intuition; he describes the character of a system of Axioms in that they must be independent, as few as possible, and yet sufficient for the establishment of the properties of figures without tacit assumption of other axioms. No definition or axiom is satisfactory which contains any notion not previously cleared up, or anything to be afterwards deduced. Any geometrical figure regarded as existing in the space of intuition may be replaced by a well-defined mental object or "Form," in the sense of the word fully described in the Introduction. The geometrical axioms are replaced by hypotheses serving to discriminate among possible forms or possible formal relations. Intuition is taken as a guide to the choice of hypotheses. The distinction is drawn between Abstract Geometry and its practical applications, and it is pointed out that there may be axioms of great importance for the latter which are useless restrictions in the former: such axioms are that the space of intuition is the Euclidean space form, and that the space of intuition has three dimensions. For our author all conceivable space forms are in theory equally admissible, and the number of dimensions of space is unlimited. The straight line, the plane, space of three or n dimensions, are all regarded as existing in the General Space. His method is the method of Pure Geometry, and his work is free from any trace of axes, coordinates, and Algebraic processes. Apparently this method has not previously been applied to the discussion of space of more than three dimensions.

A reader who approached Prof. Veronese's book in the hope of finding a logical development in purely Geometrical form of the theories of the non-Euclidean Geometry would be disappointed, for the work is throughout subordinated to the Euclidean system; nor would the reader be better satisfied if he sought merely for the logical establishment of the Euclidean system, for it is throughout treated as a limit included in a more general possible system. It is well known that the Euclidean Geometry is the limiting form between the Hyperbolic and Elliptic Geometries, and this is the case whatever more particular character we attribute to either of these Geometries. Hyperbolic Geometries differ with the form chosen for the "Absolute," there are two Elliptic Geometries according as two straight lines have one or two common points. All these systems have Euclid's system as a limit. In the elements of an Abstract Geometry developed in an orderly way we shall be presented time after time with a choice of hypotheses. Our choice at any time will determine to some extent the space form of which we treat. Our series of hypotheses will limit us to a particular space form. If one of our hypotheses is the existence of straight lines, we shall come upon the Euclidean system or a non-Euclidean system having the Euclidean as its limit. We may state at once that Prof. Veronese's hypotheses lead him to a system which, in an absolute sense, is the so-called Spherical Geometry, as distinguished from the Elliptic Geometry proper. According to this system two straight lines cut in two "opposite" points, and the length between opposite points is constant. This, however, is only true in an "absolute sense," the length in question being actually infinite in comparison with any perceivable length treated as a unit. The doctrine of the "actually infinite" is that

laid down in the Introduction. The artifice of using two units, the finite or Euclid's unit, and the infinitely great or Riemann's unit, is an essential part of the theory, and is referred to in the title of the book.

Let us now look a little more closely at the abstract development of Geometry as treated by Prof. Veronese. Such notions as "point" and "line" are suggested by simple intuitions; abstractly considered what are they? The point is simply the fundamental element of geometrical forms. It is an axiom that there are different points, but all points are identical. A straight line is a continuous point-system of one dimension, identical in the position of its parts, and determined by two of its points. Here it is to be observed that the straight line is not necessarily determined by *any* two of its points. It is an axiom that any point on the line and any point off the line determine only a single straight line. Hence if there are two or more points on a straight line by which it is not determined, any straight line through one goes through all. So far the Euclidean and non-Euclidean systems are not in any way discriminated. The choice of a system, excluding the Hyperbolic Geometry, is made by means of an hypothesis concerning different units. Let straight lines be drawn from a point, and any length in one of them chosen as a unit. On each of them there will be a "range of the scale" with that line as unit, and, as in the Introduction, there will be points outside the range of the scale. The points within the range of the scale form the finite domain about the point. The points at an actually infinite distance of the first order form the domain of the infinitely great of the first order. There can thus be a number of domains of infinitely great or infinitely small order of the space about a point. Suppose a point A taken on a straight line, and a point R outside it, and let the distance between them be chosen as a unit. Then if we join R to a point B on the line at a finite distance from A, the lines R B, A B are different relatively to the unit; if the distance A B is infinitely great in comparison with A R, they coincide relatively to the unit. The hypothesis which excludes the Hyperbolic Geometry is that two straight lines going from a point which in any domain are different relatively to the unit of that domain will not in any other domain coincide relatively to the unit of that domain, and it is proved that, on this hypothesis, two straight lines joining a point R to points at infinity in opposite directions on a line A B lie in the same straight line through R.

A point-system, defined as a straight line is defined, may be closed or open. In the former case starting from one point, and going through the system continuously in one direction, the point of starting will be ultimately arrived at; in the latter it will never be arrived at. If we assume the straight line open, and make the hypothesis just now described to exclude the Hyperbolic Geometry, we shall come to an absolute Euclidean Geometry. If we assume the straight line closed, but its entire length actually infinite in comparison with a perceivable unit, we shall come to a Spherical or Elliptic Geometry which coincides with the Euclidean in the domain of the perceivable unit about any point. This is the assumption chosen by Prof. Veronese. But there is still a choice open between the Spherical and the properly Elliptic Form. As mentioned above, the former is chosen by means of the hypothesis that a straight line contains pairs of points by which it is not determined. This hypothesis is adopted to avoid the kind of complication which occurs in the Elliptic Geometry, and which may be associated with the statement that the plane of the Elliptic Geometry is "unifacial" in the sense in which that word is used in Geometry of Position; but it is pointed out that for the purpose of obtaining a system including Euclid's as a limit, the hypothesis is a pure convention.

We have described the foundations of Prof. Veronese's system at considerable length, because it is by these that his system must be judged. For the subsequent developments it will be almost sufficient to say that they are clear and orderly, and, in places, very interesting. The construction of the plane by means of a pencil of rays meeting a straight line, leads to the essential properties of the plane and of plane figures. The like method by means of the "star" of rays from a point outside a plane to points on the plane, leads to the properties of figures in space of three dimensions. The word Star (*Stern*) is introduced in place of the older Sheaf of rays (*Strahlen-Bündel*). The construction of space of four dimensions is made by means of a star of rays from a point outside a space of three dimensions to the points within it, and so on for a space of any number of dimensions. Abstractly considered there cannot be in the nature of the case any restriction of Geometric Forms to space of three dimensions. All the forms—the straight line, plane, &c.—are treated first as Euclidean and afterwards as "complete" in the sense of the Spherical Geometry above described. The Euclidean forms first considered are regarded as the parts of the complete forms in the domain of the perceivable unit.

The use of more than one unit precludes the application to geometric magnitude of the axiom V of Archimedes; but there is another principle which has frequently been supposed to lie at the basis of Geometry with which our author also dispenses, we refer to the Principle of Superposition, or Motion without Deformation. He points out that, although this principle has been very extensively used as the test of equality, it yet involves in its statement the notion of equality, albeit in a limited form, and, as a test of equality, it is thus without meaning in an abstract sense. By placing the notion of equality of geometric magnitudes, or, as he says, identity of figures, on a different footing, he is enabled to prove the equality of congruent and symmetric figures, and to establish the idea of motion without deformation by means of continuous systems of identical figures.

The reader will see that the purpose of the book is not didactic, but the author hopes to produce a book adapted for learners, founded on the principles laid down, but limited to the Euclidean domain of a single unit. We shall look forward with much interest to its appearance. The indictment of Euclid is perhaps not yet complete, as almost every advance in Geometry throws light on some weakness in his logic, or defect in his method; but it is not too much to say that no well-reasoned didactic treatise on Elementary Geometry has yet appeared. In the meantime those who have studied the subject in the existing defective works will do well to clear their ideas by reading at least some parts of Prof. Veronese's.

A. E. H. L.

THREE GREAT EMPIRES.

Primitive Civilisations; or, Outlines of the History of the Ownership in Archaic Communities. By E. J. Simcox. Two vols. (London: Swan Sonnenschein and Co., 1894.)

THE two stout volumes which represent the work before us cover so wide a field, that it is practically impossible to enter into any detailed criticism of their contents. All that it is possible to do within the limits of a review is to give a summary of the facts and arguments which they contain, and to remark in general terms on the views of the authoress.

Beginning in chronological order, Miss Simcox opens with a description of the civilisation of ancient Egypt, and no plainer evidence can be afforded of the great strides which have of late been made in Egyptology than that which is supplied by her book. Until quite modern times Herodotus may be said to have been the chief authority on Egyptian history; but the recent excavations, and the increased and increasing power which the key of the hieroglyphics has placed in our hands, has opened a new and wide knowledge of much that relates to ancient Egypt. One primary point on which Miss Simcox dwells has yet to be proved to demonstration. This is the question—whence and by what route or routes the earliest Egyptians reached the banks of the Nile? It is generally admitted that they were immigrants from Asia, and three roads leading to the land of the Pharaohs were therefore open to them. Some have supposed that, having wandered to the south of Arabia, they crossed into Egypt in the neighbourhood of the Straits of Bab-el-Mandeb; others hold that the route across the Red Sea to Kosseir was the one which they followed; and yet others are of opinion that they crossed by the Isthmus of Suez. Against this last route there is much to be said; but one fact which Miss Simcox mentions appears to give it some support. As has lately been shown by Dr. Terrien de la Couperie, the Chinese word for "north" originally signified nothing more nor less than "back," and the name of the "south," "the front." In the case of the Chinese these terms are peculiarly appropriate, since having entered the country of their adoption from the north, that point of the compass would be at their back, with the south fronting them. The early Egyptians, Miss Simcox tells us, applied precisely the same terms, "back" and "front," to Lower and Upper Egypt, and these expressions would, at first sight, lend colour to the theory that the Egyptians, like the Chinese, entered the new country from the north. It is possible that some fresh discovery may throw a new light on this problem, and until it does we must be content to possess our souls in patience.

We may say at once that Miss Simcox's book is extremely interesting. The facts are marshalled in good order, and her literary style is clear and graphic. The portion of her work which will probably attract the greatest share of attention is that in which she draws comparisons between the early histories of Egypt and China. Many of the details of Egyptian history in the earliest times find parallels among the primitive Chinese States. The hereditary princes of Egypt find their counterparts in the feudal princes of China; while the book of Kaquimna and the lessons of Ptah-hotep remind one irresistibly, both in matter and manner, of the Chinese classics. Both countries were essentially democratic in their institutions; in both high offices were open to all, and the voice of the people was in matters of administration the ultimate court of appeal. These and many other points of agreement are dwelt upon by Miss Simcox at some length; while, at the same time, she has reproduced from the pages of Maspero, Lepsius, Erman, Birch, Eisenlohr, Griffith, and others, a full and graphic account of the manners and customs of the ancient dwellers by the Nile.

The civilisation of Babylonia follows after that of Egypt, and much that the authoress says about it points inevitably to a close connection between the two empires.

As yet we are not in a position to say which is the earliest; and this is another point upon which it is necessary to suspend one's judgment. Comparatively little at present is known of that great centre of culture in Babylonia. And it may be, as Miss Simcox seems to imply, that the primitive civilisations all sprang from a common nursery between Khotan on the east and the sources of the Karun on the west. So far as China is concerned, we have preserved in the literature of the country far fuller and more complete information, and Miss Simcox has therefore been able to fill a whole volume with matters pertaining to the people of the Flowery Land. Of course, all her information is second-hand, and, fortunately, she has for the most part consulted trustworthy authorities. So much, however, cannot be said of some few of the works from which she occasionally quotes, and the result is that the picture she draws is on some points more ideal than real. She has taken the Chinese too much at their own estimate, and has accepted their high-sounding professions as representing solid verities. In this way she has succeeded in throwing a *couleur de rose* over everything Celestial.

According to her, the people are everywhere well-to-do, justice is evenly administered, honesty prevails, education is universal, and even girls up to a certain age enjoy the same educational privileges as their brothers. A practical acquaintance with the country makes large inroads on these deductions. To anyone who has passed beyond the neighbourhoods of the treaty ports into the interior, it is manifest that the great bulk of the people live perpetually on the verge of starvation. The least failure of crops or disturbance of trade produces widespread misery and destitution, and the want of intercommunication, which Miss Simcox does not regard as serious, is consequently one of the most pressing needs of the empire. The administration of justice is an open shame, and the provision, of which Miss Simcox approves, by which criminals are compelled to confess their guilt before punishment can be inflicted upon them, is productive of great cruelty and flagrant wrong. As to education, it has recently been officially stated by the Commissioner of Imperial Customs at Chefoo, that in the surrounding province—the province of Confucius and Mencius—only about 30 per cent. of the men can read and write; "of these, 2 per cent. can compose well, 8 per cent. fairly well, and 10 per cent. conduct commercial correspondence, while the knowledge of the remaining 10 per cent. is very slight. Of the women, a very few, belonging to the richest families (perhaps 500

in the whole province), can read and write a little, but the number of those who can even read at all is small, being, if anything, over-estimated at 1 in 1000."

In a work dealing with such wide and far-reaching subjects, it is quite impossible that an author should not occasionally be led astray. Truth compels us to admit that Miss Simcox is not an exception to this rule, but she has yet succeeded in producing an extremely interesting and able work, and one which sums up with clearness the current knowledge we possess of the civilisations of these three great empires.

OUR BOOK SHELF.

Celestial Objects for Common Telescopes. Vol. ii. By the late Rev. T. W. Webb. Fifth edition. Revised and enlarged by the Rev. T. E. Espin, M.A., F.R.A.S. Pp. 280. (London: Longmans, Green, and Co., 1894.)

WE have already noticed (NATURE, vol. xlix. p. 339) the first volume of this edition of Webb's famous "Celestial Objects." The volume under review completes the work. All astronomers are familiar with this guide to the starry heavens, and most are agreed in thinking that the preparation of a new edition could not have been placed in better hands than Mr. Espin's. It is always a risky proceeding to put new wine into old bottles, nevertheless, in the case before us, an analogous task has been successfully accomplished. Substantial additions have been made in the new volume. All double stars having primaries above magnitude 6·5, and distances less than twenty seconds of arc, have been included. After lists of the binary and double stars in each constellation stars with remarkable spectra are placed. With the latter are arranged variable stars, and then follow the positions and descriptions of conspicuous nebulæ and clusters. Altogether the volume contains the places of 2272 double stars, 629 stars with remarkable spectra, and 276 nebulæ; a total of 3177 objects. The Right Ascensions and Declinations have been brought up to 1900.

It is almost unnecessary to commend the book to practical astronomers, for they are all acquainted with its merits. Certainly no possessor of a workable telescope can dispense with this trustworthy guide to celestial sights.

Ponds and Rock-Pools, with Hints on Collecting for, and the Management of, the Micro-Aquarium. By Henry Scherren. (London: The Religious Tract Society, 1894.)

THE chapters of this little book appeared originally in the *Leisure Hour*, but have been, we are told, "considerably enlarged and very carefully revised." The work is divided into six chapters, devoted respectively to the subjects of "Pond and Rock-Pool Hunting," "The Beginnings of Life," "Sponges and Stinging Animals," "Worms," "Starfish, Anthropods, and Molluscs," and "The Micro-Aquarium." The author has a pleasant, straightforward style, and has avoided as far as possible the use of high-sounding names and language calculated to deter his unscientific readers from taking up the study of the contents of "Ponds and Rock-Pools." His task has been made considerably easier by the insertion in the text of some sixty-six very creditable figures, and he has produced a book full of helpful hints to the young collector, and one which should, we think, have the effect of causing many to strive to know more about the hidden beauties of nature. The general get-up of the book is everything that could be wished.

Newfoundland as it is in 1894: A Handbook and Tourist's Guide. By Rev. M. Harvey, LL.D., F.R.S.C. (London: Kegan Paul, 1894.)

THIS book does not require a lengthy notice in these columns, being interesting more from a commercial than a scientific point of view. The author, who has lived for more than forty years in the colony, is evidently quite an enthusiastic lover of Newfoundland, and has written this handy volume for the purpose of making the country better known, and attracting to it the attention which it deserves, and which the author considers it has failed to receive in the past. Mr. Harvey has certainly done his best to alter this condition of things, and has brought together in a readable form a great deal of information respecting the physiography and topography of the island, its roads and railways, agricultural resources and forest wealth, minerals, fisheries, characteristics of the people, and other facts likely to be of service to the intending visitor or settler.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Logic of Weismannism.

SOME time ago, when I read an account of Prof. Weismann's experiment on the larvæ of blow-flies, as described in his Romanes lecture, a criticism of it occurred to me, which I have recently communicated to Mr. Herbert Spencer. This was that in the experiment the quantity of food (flesh) was diminished, while the nature of it remained the same; whereas in the case of bees it is well known that the difference between the food of the worker larvæ and the royal larvæ is one of quality, not of quantity. The royal food is pollen, and is highly nitrogenous, while the food of the worker larvæ is chiefly honey. In the case of Termites, Grassi has found that the fertile individuals are fed during development on the secretion of the salivary glands of other individuals, while the sterile forms are supplied only with macerated wood-dust.

But I was not aware, until I procured the printed version of the lecture, that Weismann had actually mentioned facts proving the importance of nitrogenous food in relation to the reproductive organs of the blow-fly. He seems serenely unconscious that these facts, mentioned in the notes to his lecture, entirely neutralise the force of his argument in the text. In note II he states that his blow-flies when abundantly fed on carrots and sugar, laid no eggs for more than a month, but as soon as meat was supplied they sucked it greedily, and laid a great number of eggs a week afterwards. In later experiments, when the flies were fed from the first with sugar and meat juice, the deposition of eggs commenced ten days after the metamorphosis. Weismann infers that rich food is necessary in the imago stage if the egg-cells are to ripen, and adds that in the case of bees the queen lays eggs because supplied with nitrogenous food in the imago state, while the workers are poorly fed. These remarkable facts concerning the relation between egg-laying and nitrogenous food in the adult blow-fly strongly suggest that if the larvæ were deprived of nitrogenous food during development, the ovaries would not be perfectly developed.

Weismann contends that the bee has the specific property of responding to imperfect nutrition in the larval state by the imperfect development of the ovaries. As proof of this, he states that blow-fly maggots occasionally starved, but fed exclusively on meat like those which were not starved, laid normal eggs in normal abundance, and were only smaller in size. The evidence is quite irrelevant. The point is that the larva of the worker-bee is supplied with a non-nitrogenous diet, that of the queen bee with one highly nitrogenous. What is required is evidence that the larva of the blow-fly can fully develop its ovaries when deprived of nitrogenous food. Instead of this, Weismann supplies the information that the blow-fly when reared on a restricted quantity of nitrogenous food, can lay eggs if further fed with proteids in the imago stage, but if

deprived of proteids in the imago stage it lays no eggs. Of the anatomical condition of the reproductive organs in the flies experimentally reared no evidence is vouchsafed.

Plymouth, September 15. J. T. CUNNINGHAM.

"Darwinism is not Evolution."

I WAS very much struck—having heard the admirable reply which Prof. Huxley gave to Lord Salisbury on the evening of August 8—to find a passage in "Darwin's Life and Letters" (vol. iii. p. 13) which is the exact counterpart of the chief point in Huxley's retort. Darwin writes to Lyell (March 12, 1863): "I must feel convinced that at times . . . you have as completely given up belief in immutability of specific forms as I have done. . . . The more I work the more satisfied I become with variation and natural selection, but that part of the case I look at as less important, though more interesting to me personally."

It was whispered in Oxford that Huxley had spoken of Darwinism rather lightly in comparison with evolution. The above-quoted passage shows that even in this respect Darwin himself had set the example.

I have reason to believe that these illustrious examples will not be very generally followed. A. A. W. H.

Utrecht, September 11.

Extraordinary Phenomenon.

HAVING recently had before me your number of the 6th inst., I feel very desirous to bring under your notice, for insertion in your journal, the description of a most extraordinary and singular phenomenon as was observed by me at Llanberis, N. Wales, on Sunday, August 26 last, about 10.30 p.m.; especially as I perceive that the time of my observation coincides precisely with the time recorded in that number by John W. Earle, at Gloucester, describing his observation of a remarkable meteor which he discovered.

I was outside the hotel in Llanberis at 10.30 p.m. admiring the lustre of the stars—for it was a cloudless night—when, gazing upwards into the region of Cassiopeia, I was startled by a sudden flash from a brilliant effulgence of white light situated proximately to the two stars of greatest magnitude in that constellation, which immediately resolved itself into a clearly defined disc, about three times the diameter of Jupiter. After a brief interval I observed a body of brilliant orange colour discharged from the disc, which was projected directly towards Perseus. This body assumed a form resembling an elongated flatfish, but terminating in a point, the disc forming a nucleus to the apparition, which was marvellous to behold; but its visibility proved to be only of short duration, for the white disc, or nucleus, suddenly disappeared, leaving the orange-coloured mass quiescent for about half a minute, and then I saw it fade away gradually, and it vanished out of my sight.

The appearance of this strange body did not occupy more than five minutes of time; its dimensions in length I estimated was about fifteen degrees of arc. I likewise noticed an important fact—that it evidenced no motion in space.

During my professional career, including Arctic and Equatorial services, a great part was spent in nightly watchings, in which all sorts of meteoric phenomena came under my notice, yet I never beheld one which manifested such marked singularity and distinctiveness combined. I could only regret that no one was at hand to affirm what I saw.

With reference to the meteor observed by John W. Earle in Ursa Major, I wish to mention that a building excluded that constellation from my sight, therefore it establishes a very interesting and important fact that these two extraordinary phenomena, one in Ursa Major, the other in Cassiopeia, were so distinctly notified by two observers so remote from each other at the very same moment.

ERASMUS OMMANNEY, Admiral.

29 Connaught Square, W., September 24.

"Aurelia aurita."

AT the Plymouth Laboratory, in July last, I examined 383 adult specimens of *Aurelia aurita*, and found eight specimens (2·08 per cent.) showing a numerical variation in the generative sacs and buccal arms.

One specimen with 3 generative sacs, 3 buccal arms, and 9 tentaculocysts. Three specimens with 3 generative sacs, 3 buccal arms, and each one has traces of a fourth generative sac and a fourth arm. Two have 8 tentaculocysts, and one has 10 tentaculocysts. One specimen with 5 generative sacs, 5 buccal arms, and 8 tentaculocysts. Three specimens with 6 generative sacs, 6 buccal arms; two have 11 tentaculocysts, and one has 12 tentaculocysts. Six specimens with the normal number of buccal arms and generative sacs show a variation in the size and shape of the sacs. There appears to exist a correlation between the generative sacs and buccal arms, but the tentaculocysts vary independently of the other organs.

I found 87 specimens (22·8 per cent.) showing a variation in the number of tentaculocysts. Twenty specimens possess less than the normal number, and the remainder show an excess. The range of variation extends from 6 to 15 tentaculocysts.

EDWARD T. BROWNE.

University College, London, September 15.

Science in the Medical Schools.

IN the issue of NATURE of September 20, I notice a table of the scientific classes which are to be given in the medical schools of Great Britain during the session 1894-95. In this table I find that the subject of "biology or zoology" is indicated by a cross (×) as being taught in all the medical schools of Scotland with the exception of the University of Aberdeen. You will, doubtless, allow me to point out that in this matter Aberdeen is in precisely the same position as Edinburgh and Glasgow. A course of zoology is delivered in the University of Aberdeen in the winter session, and a second course in the summer session, and there is, in addition, a course of practical zoology.

University, Aberdeen. H. ALLEYNE NICHOLSON.

ON THE DOCTRINE OF DISCONTINUITY OF FLUID MOTION, IN CONNECTION WITH THE RESISTANCE AGAINST A SOLID MOVING THROUGH A FLUID.

§ 1. THE doctrine that "discontinuity," that is to say finite difference of velocity on two sides of a surface in a fluid, would be produced if an inviscid incompressible fluid were caused to flow past a sharp edge of a rigid solid *with no vacant space between fluid and solid* was, I believe, first given by Stokes in 1847.[1]

It is inconsistent with the now well-known dynamical theorem that an incompressible inviscid fluid initially at rest, and set in motion by pressure applied to its boundary, acquires *the* unique distribution of motion throughout its mass, of which the kinetic energy is less than that of any other motion of the fluid with the same motion of its boundary.

§ 2. The reason assigned for the formation of a surface of finite slip between fluid and fluid was the infinitely great velocity of the fluid *at* the edge, and the corresponding negative-infinite pressure, implied by the unique solution, *unless the fluid is allowed to separate itself from contact with the solid.* This an inviscid incompressible fluid certainly would do, unless the pressure of the fluid were infinitely great everywhere except at the edge. In nature the tendency to very great negative pressure arising from greatness of velocity of a fluid flowing round a corner is always obviated by each one of three defalcations from our ideal:—

(I.) Viscosity of the fluid, preventing the exceeding greatness of the velocity.

(II.) Compressibility of the fluid.

(III.) Yielding-ness of the outer boundary of the fluid.

§ 3. Defalcation (I.) is in many practical cases largely operative when air is the fluid; but (II.) is also largely operative in some very interesting cases, such as the *whistling* of a strong wind blowing round a sharp corner or through a chink; the blowing against the sharp edge in the embouchure of an organ-pipe, and in the mouth-

[1] "Collected Papers," vol. i. pp. 310, 311.

piece of a flageolet or of a small "whistle"; and the blowing across the end of a tube or a hole in the side of a tube, to cause a key or a flute to sound.

§ 4. Defalcation (III.) is largely operative, and (II.) but little, in many practical cases of most common occurrence in the flow of water. It is probable that much of the foam seen near the sides and in the wake of a screw steamer going at a high speed through glassy-calm water, is due to "vacuum" behind edges and roughnesses causing dissolved air to be extracted from the water. A stiff circular disc of 10-inch diameter, and 1/10 of an inch thick in its middle, shaped truly to the figure of an oblate ellipsoid of revolution would cause a vacuum[1] to be formed all round its edge, if moved at even so small a velocity as 1 foot per second under water of any depth less than 63 feet; if water were inviscid: and at greater depths the motion would, on the same supposition, be wholly continuous, with no vacuum, and would be exactly in accordance with the unique minimum energy solution.[2]

While the velocity of the fluid across the equator is 63·7 feet per second, the velocity across each of the two parallel circles whose radii are 4·218 inches (the radius of the equator being 5 inches) is only 1 foot per second.

§ 5. The exceedingly rapid change of shape of the fluid flowing across the equatorial zone between these circles, with velocity at the surface augmenting from 1 foot per second to 63 feet per second in advancing over a distance of less than ·85 of an inch of the surface from one of the small circles to the equator, and diminishing again from 63 to 1 from the equator to the other parallel, in a small fraction of a second of time would, if the fluid is water or any other real liquid, give rise, through viscosity, to forces greatly diminishing the maximum velocity, and causing, through fluid pressure, the motion of the water to differ greatly from that of the minimum-energy solution, not only near the equator, or in its wake, or over the rear side of the disk; but over all the front side also, though no doubt much more on the rear side and in the wake, than on the front side, and in the fluid before it.

The viscosity would also, at less depths than 63 feet, have great effect in keeping down the maximum velocity; and it is possible that even at 10 or 20 feet a greater velocity than 1 foot per second might be required to make vacuum round the equator of our disc of 10 inches diameter and the $\frac{1}{2000}$ of an inch radius of curvature which its elliptic meridianal section gives it. But it seems quite certain that there must be much forming of vacuum, and consequent extraction of air and rising of bubbles, to the surface, from the somewhat sharp corners, and roughnesses, of iron, in the hull of an ordinary iron sailing ship or steamer, going through the water at twelve knots (that is, 20 ft. per second). KELVIN.

(*To be continued.*)

[Correction on previous short article, "Towards the Efficiency of Sails, &c." In last line but two, for 2π substitute $\frac{1}{2}\pi$. In last line but one, delete 4, and for 8 substitute 32.]

[1] Single word to denote space vacated by water.

[2] From the elementary hydrokinetics of the motion of an ellipsoid through an inviscid incompressible fluid, originated by Green, who first gave the solution for the case of translational motion of the ellipsoid, we know that, if θ denoting the angle between the axis of an oblate ellipsoid of revolution, of which the equatorial and polar areas are a, b, the velocity of the fluid flowing over this point of the surface is

$$\frac{(a^2 - b^2)\mathrm{V} \sin\theta}{b\left\{b + \dfrac{a^2}{\sqrt{(a^2 - b^2)}} \sin^{-1}\dfrac{\sqrt{(a^2 - b^2)}}{a}\right\}},$$

if the velocity of the fluid at great distances from the solid is V, and in parallel lines, and the solid is held fixed in the fluid, with its axis parallel to these lines. Taking $a = 100b$ in this formula, we reduce it to $\frac{200}{\pi}\mathrm{V}\sin\theta$ approximately within 1 per cent.; and taking $\sin\theta = 1$, and $\mathrm{V} = 1$ foot per second, we find 63·7 feet per second for the velocity across the equator. Hence the gravitational head corresponding to the "negative-pressure" is $(63{\cdot}7^2 - 1^2)/64{\cdot}4$, or very approximately 63 feet, which proves the statement in the text.

SCIENCE, IN SCHOOL AND AFTER SCHOOL.

IT is an unfortunate accident of the conditions under which instruction in science has grown up, that in speaking of science teaching two essentially dissimilar things should be confused. This confusion has very seriously affected—and still affects—the development of method in this country. It arises from the fact that, twenty or thirty years ago at least, the ordinary schoolmaster was quite without the knowledge necessary to teach science, and that even when his scientific knowledge was a measurable quantity, that ignorance of psychology which was and which remains one of his most constant characteristics, rendered him incapable of innovations upon the tradition of mental training he cherished. Consequently what knowledge people obtained of the growing body of science came after the elementary stage of education was over, when their minds and senses had already received a considerable amount of cultivation and were, for good or evil, definitely developed in a prescribed way. The teaching given, therefore, did not aspire to be so much educational as *instructive*; it made the best of a bad job, and without any belated attempts to alter the fundamental intellectual mechanism, placed therein so much of the new facts and views as the circumstances permitted. It was addressed primarily to adolescence and to the adult, its methods were by lecture, diagram and text-book, and the written examination or a practical examination, turning chiefly on the identification of specimens or the interpretation of diagrams, was the adequate measure of its value. Such teaching can affect the taught only through their opinions and knowledge; it can discover scientific capacity, but it can neither develop nor very largely increase it, because it comes too late in the mental life. It is typically represented by the innumerable classes over which the Science and Art Department presides.

On the other hand, we have the science teaching that is *educational*, that takes the pupil still undeveloped and trains hand, eye, and mind together, enlarges the scope of the observation, and stimulates the development of the reasoning power. Such science teaching occurs at present most abundantly in theoretical pedagogics. It is, however, undoubtedly the proper science teaching for the school, if science is to have a place in the school. For it is universally conceded nowadays that the school is a training place, that there the vessel is moulded rather than filled, and that the only justification for the introduction of science is its educational value. Equally indubitable is it that it should be confined to school limits. An attempt to make the adult science teaching educational in the same sense, would be—to complete the image—extremely like putting a well-baked—if imperfect—vessel back upon the potter's wheel.

Now, hitherto the chief influence of this confusion has been to hamper truly educational science teaching in schools. Those who had as adults studied science under the Science and Art Department, or in University lecture theatres, took their text-books and the methods under which they had acquired their knowledge into the school, where the conditions were altogether different. The course of science lessons began as a lecture in which the class listened to colourable imitations at second or third hand of this or that eminent exponent of scientific theory. The more discerning teachers after a time realised the futility of requiring genuine lecture notes from such immature minds, and supplied the deficiency *by dictating* a colourable imitation. They also provided copies on the blackboard for such original sketches as were required, and indeed went to very considerable pains to keep the outward appearance of the lecture system intact. Examiners of schools—being selected without the slightest reference to their capacity to examine—fell very readily into this view, that school science-teaching was adult

science-teaching in miniature; as some parents hold that infant costume should be a simple and economical adaptation of the parental garments. And so an elaborate system of lecturing, note dictating, "model answer" grinding, has been evolved, which is not only not educational and a grievous waste of the pupils' energies, but which seriously discredits the claims of science upon the school time, in the eyes of ordinary educated people.

This has been particularly the case in many middle class schools, though the recent abolition of the second class pass in the May examination has done much, as the Forty-first Report of the Department shows, towards mending the mischief. In connection with countless higher grade and small grammar schools, classes, containing as a rule only elementary pupils, and aiming really only at second class passes, have been organised from year to year. Not only was the science-teaching given in the evening classes, but a considerable portion of the daytime was devoted to model answer drill and to mechanical copying out from the text-book. The minimum of apparatus required by the Department formed a picturesque addition to the schoolroom. This discipline resulted in remunerative grants for second class passes, but it resulted in very little else, except perhaps a certain relaxation of the pupil's handwriting and a certain facility in the misuse of scientific phrases. The certificates were framed and glazed, the teacher added a few modest comforts to his home, and there the matter ended.

The examinations of the Science and Art Department were scarcely to blame in this matter, although the blame has been generously awarded them. The Science and Art Department is a large and convenient mark, it is perfectly safe to throw at, and to attack it has something of the romantic effect of David against Goliath. But we must remember that its classes were primarily, as they are still in intention, continuation and adult classes, an outcome of the Mechanics' Institute movement, and it was an unforeseen accident, and one the full bearing of which only became apparent in the course of years, that they should so seriously affect the teaching of middle-class, and even of the higher standards of elementary, schools. For their proper purpose as a test of lecture teaching, the departmental examinations are generally efficient. Far more blameworthy are examining bodies whose work is specially directed to school needs. The College of Preceptors, for instance, while subsidising lecturers upon Educational Theory, has done nothing to promote practical work in schools, and many of its examinations set a premium upon that vicious lecture and text-book cramming which educational theory condemns. And in public schools over which the Department has no influence, young gentlemen from the older universities, beginning educators without of course the faintest knowledge of educational technique, set up precisely the same imitation of the professorial course. We have in consequence such a standing argument against science teaching as that naïve testimony of a prominent head-master, that he found boys who had followed the classical course for some years, and who then took up "science as beginners," speedily outstripped those who, to the exclusion of literary work, had been engaged during the same time in what he regarded as scientific studies.

So far the confusion between the two forms of elementary instruction has hampered science-teaching. But there can be no doubt that the educational reformer is abroad. A large, if somewhat inchoate, body of criticism has grown up, and good resolutions in the matter are epidemic. A really educational scheme of instruction in physics and chemistry now exists, having its base upon the Kindergarten, and developing side by side with elementary work in mathematics. Mr. Earl's recently published book upon Physical Measurements is an admirable exposition of what is here intended by educational science-teaching. In this, information is entirely subordinated to mental development. His course is devoted to the measurement of space, mass and time, and to the observation and methods of recording various changes involving precise determinations. The first exercise requires the pupil to "measure the size or dimensions in inches of the paper on which you are writing, using for your standard a strip of paper one inch in length, and which you have divided into halves, quarters, and eighths"; and the book concludes with experiments upon torsion and the rotation of suspended bodies. The course must inevitably constitute a firm foundation of definite concepts, and develop a clear and interrogative habit of mind. It marks the line along which school science teaching must move in the future, if it is to attain that predominance which its advocates claim for it. Yet at the same time it may not be premature to notice that the new movement has its dangers.

These dangers arise from the confusion between the two distinct forms of science-teaching whose existence is necessitated by the present condition of things. In the past the error has been to treat children like adults; in the future it may be that adults will be treated like children. Such exercises as the one we have noticed, are excellent in developing concepts, but scarcely anything could be devised more irksome and exasperating to a mind already provided with a basis of definite ideas. Nothing, for instance, could be better calculated to discourage an intelligent student of eighteen or nineteen, curious about physics, than a day or so spent in manufacturing an unreliable millemetre scale. The problems of the science are already more or less vaguely in his mind, and there is every reason why these should be made the starting-point. To produce an intellectual parallel to the spiritual re-birth, is as impossible as it would be to refer an unsatisfactory chicken back to the egg to reconsider its ontogeny. We have now, and shall have for an indefinite number of years, to provide for the needs of a great number of people whose intellectual development is nearly or quite at an end, whose curiosity about nature is already aroused, and whose practical needs are also pressing for scientific information, and yet who are ignorant of any but the veriest commonplaces of science. For them the Science and Art Department classes were designed and are well adapted. It will be an unfortunate thing if the criticisms of the educational reformer should so far overshoot the mark as to affect their instruction. Yet one might suggest that a downward age limit, similar to that of the London University Matriculation, might save many a schoolmaster from the temptations of the possibility of grant-earning—a temptation, however, from which the abolition of the second class in the elementary stage has already to some extent relieved him.

H. G. WELLS.

WITH PROF. HEIM IN THE EASTERN ALPS.

THE excursion with Prof. A. Heim, of Zürich, which came to a happy end on September 15 at Lugano, was one full of interest to students of tectonic geology. It afforded those who were fortunate enough to take part a rare opportunity of seeing in the field some of the classic sections with which the name of Heim has been for many years associated, and, better still, of seeing the genial author of the "Mechanismus der Gebirgsbildung" himself climbing his native Alps as nimbly as a chamois, and expounding his own work face to face with the hard facts on which his conclusions have been based. The party, numbering at the outset twenty-three, left Zürich on the 3rd, after the close of the Geological Congress, and took train to Appenzel, spending the first night at Weissbad, a village nestling peacefully beneath the rugged peaks of the Säntis Range. This magnificent buttress of the

Eastern Alps consists of a series of steep compressed folds of cretaceous strata rising through the Flysch and Molasse conglomerates that form the lower spurs to the north, and the summit of Säntis is the highest of one of the sharp anticlines of the range. Finer examples of sharp anticlinal peaks and ridges separated by equally acute synclinal gorges it would be difficult to find, and the well-marked petrographical and palæontological character of each zone leave no room for doubt as to the sequence and structure of the different formations. The summit of Säntis (8200 feet) is an overturned anticline of Gault, covered by a thick bed of "Seewerkalk," the highest cretaceous rock in the district, and the structure is exposed in splendid cliff sections on the sides of the peak. Prof. Heim, who is an adept at drawing panoramic sketches, was anxious that we should have an opportunity of verifying his elaborate panorama of the surrounding district, but unfortunately the mist, which had come on, did not clear off the top till the afternoon of the second day, and even then we had only a short glimpse of the glorious view beneath. Two nights were spent in the inn erected by the Alpine Club, a short distance below the observatory on the summit of Säntis, and the descent to Wildhaus was made on the north side over a steep path leading across a compressed synclinal fold of Seewerkalk, where a good example was seen of the middle part of a double fold, compressed and drawn out so as to pass almost into a thrust-plane. A better example of the "Verkehrten Mittelschenkel" was, however, seen a few days afterwards in the Mattstock near Amden, where the middle members of a compressed monoclinal fold were found in normal order from Flysch to Neocomian and Gault, but drawn out and evidently much diminished in thickness. At Obstalden, on the Wallen See, a few days were spent among the rocks on either side of the lake, a recent fall of snow having obscured the sections on the higher Alps, which we had intended to visit. One of the most interesting tectonic features of this district is an important thrust-plane, traversing the face of the Leistkamm on the north side of the lake, and repeating the section of cretaceous rocks in the mountain. The thrust-plane forms a barrier to the downward passage of water through the limestones above, and its outcrop is marked by a line of springs, one of which gushes out of the cliff in a large waterfall opposite Mühlehorn. After a visit to the Mürchenstock, with its contorted anticlinal core of Permian Verrucano conglomerate and its wrinkled skin of Jurassic rocks, we made our way southward to Glarus, and the greater part of the last week was devoted to the exploration of the celebrated "Doppelfalte" of the Glärnish, in which the red Verrucano is seen to have been pushed on to the top of Jurassic and Eocene rocks exposed in a series of magnificent mountain sections. At Lochseite, near Schwanden, a few miles south of Glarus, the massive Verrucano is seen projecting in a thick ledge from the hillside over the so-called Lochseitenkalk, a crushed irregular bed of mylonised limestone resembling that found near the great thrust-planes in the North-west Highlands. At Lochseite the thrust-plane is so sharp and clear that a knife could be drawn along between the rocks on either side, and the under-surface of the hard Verrucano is slickened and polished at places as smooth as glass. A full view of this tremendous overthrust was, however, not obtained until we had climbed some 8000 feet to the crest of the Bütistock and Kalkstöckli, between Linthal and Elm, where the sharp-cut and wonderfully straight line of the great displacement was seen in profile crossing from ridge to ridge and peak to peak, and producing a marked feature in the mountain panorama. At one place where the Verrucano has been eroded off, the party rested on the smooth surface of the thrust-plane which forms the crest of the ridge, where they were photographed, with Prof. Heim standing in the midst expounding the classic sections around him. The south wing of the "Doppelfalte," or thrust-plane, was crossed between Elm and Flims at the Segnes Pass, at a height of 8615 feet, and here also the outcrop was seen in stupendous cliff sections. The overlying Verrucano being darker in colour than the limestones below, the line of displacement is everywhere very sharp and distinct. The nummulite limestone, where it approaches the thrust-plane, is drawn out and schistose, and a distinct passage was traced between unaltered nummulites and those only slightly distorted, to a rock in which they were rolled out into ribbons, which, but for the intermediate specimens, could not have been recognised as being of organic origin. The Verrucano itself is also squeezed and schistose, and the pebbles are compressed into augen, surrounded at places by sericite and mica schist. The vast interglacial (?) landslip at Flims, on the Vorder Rhein, was the last of the important objects we visited, and after a passing look at the crystalline rocks of the St. Gotthard Massiv, the party reached Lugano in detachments, where they rejoined the other members of the Congress, all highly pleased with their respective excursions across the Alps, and were greeted on their arrival by a discharge of artillery, followed in the evening by a splendid pyrotechnic display on the lake.

H. M. C.

NOTES.

THE Physical Society, which has for many years met in the Royal College of Science at South Kensington, give notice of some important changes. The Council have, after careful consideration, come to the conclusion that the meetings of the Society would be more accessible to the majority of the members if they were held in some more central situation, and the meetings will therefore henceforward be held on the same day and at the same hour as heretofore, but in the rooms of the Chemical Society, in Burlington House. All communications to the secretaries or other officers of the Society may in future be addressed to Burlington House or to the secretaries at their respective addresses as given in the list of members of the Society. The Council have also decided to initiate the publication of a series of abstracts of papers on physics, but the resources at their command being slender make it necessary to begin cautiously. At first abstracts will only be given of papers which appear in a certain number of the more important foreign magazines. They will for a time be edited by Mr. Swinburne, and will be published regularly at the beginning of each month in the form of a supplement to the Proceedings of the Society. The first number will be issued in January 1895. Should the scheme prove successful it is intended to enlarge its scope. For some time past printed copies of the more important papers have been circulated before the meetings among members who are likely to take part in the discussion on them; it has, however, been felt that cases may arise in which the author may wish that his paper should be published as soon as possible. The Council have therefore decided that, if an author so desires, and if such a course appears desirable, they will take steps to ensure that the publication of a paper is not in any way delayed in order that it may be read before publication, and that they will if necessary postpone the reading and discussion of a paper until after it has been published.

WE are informed that a petition, signed by a number of science masters, has been sent, through Sir Henry Roscoe, to the Secretary of State for War, supporting the Departmental Committee's proposal to introduce a compulsory science subject into the entrance examinations for Woolwich, which, the masters consider, will encourage thorough science teaching in the schools, and be to the advantage of education generally.

THE death is announced of Prof. Comm. Ariodante Fabretti, Director of the Historical and Philological Section of the Turin Academy of Science.

THE fourteenth annual congress of the Sanitary Institute is taking place this week at Liverpool, and was opened on Monday last by the holding of a reception in the Town Hall by the Lord Mayor of Liverpool, who is also chairman of the local committee. The new president—Sir Francis S. Powell, M.P., —afterwards delivered his inaugural address. In the evening the Lord Mayor opened an exhibition of sanitary appliances. On Tuesday the congress, divided into five sections, resumed its sittings.

THE programme for the sixty-sixth annual congress of the German Naturalists and Physicians at Vienna, from September 24 to 30, contained arrangements for no fewer than three addresses by the late Prof. von Helmholtz, all of which were to have been delivered in the general meetings. Dr. F. Klein, the Professor of Mathematics at Göttingen, has undertaken to fill one of the gaps by reading a paper upon Riemann's influence in the development of modern mathematics.

A TELEGRAM from St. Paul, Minnesota, through Reuter's special service, on Monday gave notice of the occurrence, on the evening of Friday, September 21, of a very disastrous cyclone in America. A strip of country in Iowa, Minnesota, and Wisconsin, about 200 miles in length, is reported to have been devastated, and not only was immense destruction done to property, but serious loss of life occurred, the number of persons who perished being variously estimated at figures varying from fifty-two to one hundred. The storm was accompanied by hail and torrents of rain, as well as thunder and lightning. Starting ten miles south of Spencer, North-West Iowa, the cyclone swept across the State to the north of Emmetsburg and Algona, almost wiping out of existence the town of Cylinder and laying waste the country districts in its track. Passing by Mason City, it ravaged the country to the north-west of Osage, and then changed its direction somewhat towards the north-east, crossing the Minnesota line and working great havoc in Leroy, where a fire broke out and a whole block of houses was destroyed. The cyclone swept down Spring Valley, and then turned again to the east, wrecking the hamlets of Homer and Lowther. It next crossed the Mississippi and destroyed many farm buildings near Marshland, Wisconsin. Considerable damage was also done at Dodge Centre, though it was not in the path of the main cyclone.

A CENTRAL NEWS telegram of September 25 announces that a destructive storm has occurred in Japan, by which the districts of Okita and Twate have been laid waste. Fifteen thousand houses are reported to have been destroyed, and 300 persons to have perished. Great havoc has also been wrought among the shipping.

UNIVERSITY COLLEGE, Dundee, has been benefited to the extent of some £35,000 by the bequest of the late Mrs. Margaret Harris, of Dundee.

DR. A. ZIMMERMANN has been appointed Extraordinary Professor of Botany at the University of Tübingen; and Dr. Solereder Curator of the Botanical Institute at Munich.

ACCOUNTS have been received from Prof. Stirling, F.R.S., of the safe return to Adelaide, South Australia, of the Horn Expedition for the exploration of the central portion of that country, the departure of which was announced in these columns some three months ago (p. 174). We are glad to say that considerable success has attended the whole journey, no doubt in consequence of the foresight with which preparations had been made for it; and though the work of the expedition was at times sufficiently trying, nothing that could be called a misadventure took place. Above all, there was no collision, nor indeed any trouble with the natives; and there had been good rains in the Macdonnell Ranges, the examination of the western termination of which formed the chief work to be done. The course of the expedition is briefly outlined by Prof. Stirling as follows:—From Crown Point the party traversed the Finke River to running water; thence the Palmer River to Tempe Downs, the Levi Range, Petermann Creek, and the George Gilles Range, where one section diverged to Ayers's Rock and Mount Olga, the rest proceeding westward to Lacorie's Creek and northward to Glen Edith, along Carmichael Creek to Mereenie Bluff, thence into the northern watershed following Darwent Creek to Haast's Bluff, and so eastward to Glen Helen. The united party then travelled eastward to Mount Sonder, which was ascended, and thence through the southern ranges of the Macdonnells to the Finke River and Hermannsburg. Here section one again diverged to the Glen of Palms, and another to the North Macdonnells by way of Ellery's Creek and Brinkley's Bluff to Aine Springs, where it was met by the other members of the expedition, some of whom had journeyed thither by Owen's Springs, and others by Stuart's Pass and Burt's Plains. The zoological collections formed are said to be generally good, and it has again been Prof. Stirling's good fortune to discover a new type of Marsupial. This is stated to be about as big as a small rat, with a shrew-like aspect, and a very curious flattened and fat tail. Its scientific description will probably be undertaken by Prof. Baldwin Spencer. Prof. Stirling again met with his old friend *Notoryctes*, but only obtained two examples, one of them being alive, though it soon died, notwithstanding all the care that was taken of it. The rare Alexandra parakeet was also met with; but in one locality only. Some twenty new species of terrestrial mollusks seem also to have been found, and it is expected that about seven or eight new species of plants are contained in the botanical collection, which shows a greatly extended range of many kinds that had before been supposed to be much restricted. An examination of the geological formations is adverse to the hope of metalliferous developments to the southward of the Macdonnell Ranges. It remains to be said that all the journeying was accomplished by the aid of camels, which, as before, proved themselves to be essential agents in the exploration of Central Australia. The mode in which the scientific results of the expedition are to be published is uncertain, and possibly will not be decided until the return to Adelaide of Mr. Horn, who defrayed all, or nearly all, the cost, and is accordingly to be congratulated—together with the several members of the expedition—on the success which has attended an enterprise which has been conducted with so much good spirit.

WE learn from the *Botanical Gazette* that an expedition through Eastern Africa for the collection of natural history specimens, and to secure photographs, was intended to start from Pretoria about August 1. Passing through Matabeleland, the extreme western portion of the East African Portuguese possessions, and along the western shore of Lake Nyassa, it expected to reach Zanzibar in about twelve months. The chief attention will be paid to plants and insects.

DR. A. BALDACCI is at present engaged on a botanical expedition in the Balkan Peninsula, with the especial object of exploring the mountain-chains of Albania.

WE learn, from the *American Naturalist*, that the University of Illinois is about to open a permanent station on the Illinois River, for the biological study of the flora and fauna of the waters

of that State. The laboratory will be established at Havana, and, together with the State Fish Commission, will be under the direction of Prof. S. A. Forbes. Among the problems to be investigated are the effect of the periodical overflow and recession of the river on the abundance, variety, and interaction generally of the various groups of plants and animals represented in those waters.

THE following are the subjects for competition for the two Walker Prizes in Natural History, given annually by the Boston (Mass.) Society of Natural History for the next two years:—1895: A study of the "Fall Line" in New Jersey; (2) a study of the Devonian formation of the Ohio Basin; (3) relations of the order Plantoginaceæ; (4) experimental investigations in Morphology or Embryology. 1896: (1) A study of the area of schistose or foliated rocks in the Eastern United States; (2) a study of the development of River Valleys in some considerable area of folded or faulted Appalachian structure in Pennsylvania, Virginia, or Tennessee; (3) an experimental study of the effects of close-fertilisation in the case of some plant of short cycle; (4) contributions to our knowledge of the general morphology or the general physiology of any animal, except man. The memoirs must be written in the English language, and the prizes of the value of 60 and 50 dollars respectively—the competition for which is open to all—will not be awarded unless the memoirs presented are of adequate merit. Each memoir must be accompanied by a sealed envelope enclosing the author's name and superscribed with a motto corresponding to one borne by the manuscript, and must be in the hands of the Secretary on or before the first of April of the year for which the prize is offered.

THE French Society for the Encouragement of National Industry has issued a list of prizes to be offered for competition next year in connection with chemical research. The following are among the principal subjects proposed: (1) Recent progress in the manufacture of chlorine; prize, 2000 francs. (2) The utilisation of the residues of manufactories; prize, 1000 francs. (3) A prize of 2000 francs for an experimental study of the physical or mechanical properties of one or several metals or alloys, chosen from those that are in current use. (4) 2000 francs for manufacturing in France, for trade purposes, anhydrous sulphuric acid and "smoking" sulphuric acid. A special note is given with each subject explaining the reasons for which the prize is offered, but it is understood that the money will be withheld in the event of the papers sent in not proving sufficiently interesting.

THE Entomological Society will meet on Wednesday, October 3, at 8 p.m., when the following papers will be read:—"Catalogue of the *Pterophoridæ*, *Tortricidæ*, and *Tineidæ* of the Madeira Islands, with notes and descriptions of New Species," by Lord Walsingham, F.R.S.; "Palæarctic Nemouræ," by Kenneth J. Morton.

THE majority of the Medical Schools of the metropolis will open on Monday next, and in several instances an introductory address will be dispensed with, and in its place some form of festive gathering will be held. Addresses will, however, be delivered as follows:—At St. George's Hospital, by Dr. Isambard Owen, the dean of the school; at Guy's Hospital, by Mr. Lockhart Stephens; at St. Mary's Hospital, by Dr. Scanes Spicer; at the Middlesex Hospital, by Dr. Boxall; at University College Hospital, by Dr. H. R. Spencer; at Westminster Hospital, by Mr. G. Hartridge; and at the School of Medicine for Women, by Miss M. Sturge. There will be dinners in connection with the following hospitals:—St. Bartholomew's, St. Thomas's, the London, St. George's, King's College, St. Mary's, the Middlesex, and the Westminster. At the Charing Cross Hospital there will be an evening reception, at which the prizes for the year will be distributed by Prof. Alexander Macalister, F.R.S. The Rector of Lincoln College, Oxford, will distribute the prizes at the St. Thomas's Hospital.

PROF. H. ALLEYNE NICHOLSON will deliver the Swiney Lectures on Geology on the Mondays, Wednesdays and Fridays of October, taking as his subject "The Making of the Earth's Crust." The lectures, for which no charge for admittance i made, will be delivered at 3 p.m. in the Lecture Theatre of the South Kensington Museum. The Swiney Lecturer for next year is Dr. J. G. Garson.

THE eighteenth course of lectures of the Sanitary Institute will be delivered at the Parkes Museum, Margaret Street, W., at 8 p.m. on each Monday, Wednesday, and Friday of this autumn, from Wednesday, October 17, when the opening lecture, which is specially intended for those desirous of becoming sanitary officers, will be delivered. The secretary of the institute will be happy to supply full information respecting the lectures.

THE following popular science lectures will be delivered at the Royal Victoria Hall, Waterloo Bridge-road, S.E., during the coming month:—October 2, on "Hearing," by Prof. W. D. Halliburton; October 9, on "Wonders in Nature," by Mr. R. Kerr; October 16, on "The Work of the Air on the Earth," by Mr. F. W. Rudler; October 30, on "Light, what it is and how it is measured," by Prof Carlton Lambert. There will also be a lecture on October 23, but the subject has yet to be decided upon. Each lecture will be illustrated by means of the lantern.

THE prospectus has been issued of a very elaborate "Systematic Botany of North America," to be published in seventeen vols., by a Board of editors under the presidency of Prof. N. L. Britton. The account of each natural order will be a monograph by a separate author. The area comprised in the "Flora" will be the American continent north of Mexico.

THE *Lancet* states that the Queen has been graciously pleased to intimate to Dr. Thorne Thorne, C.B., the principal medical officer of the Local Government Board, her appreciation of the services which have been rendered by the Medical Department of the Board in taking the measures which it has adopted for preventing the entrance of cholera into this country.

A NEW chemical laboratory, in connection with the Imperial University of St. Petersburg, will be opened next month. The building will contain, in addition to laboratories and a lecture theatre, dwelling accommodation for the professors and their assistants. The cost of erection has been over £25,000, four-fifths of which have been defrayed by the Minister of Education, and the remainder by the University.

A CIRCULAR has been sent to us announcing the conditional re-starting of our American contemporary *Science*, the publication of which was on March 23 suspended, owing to lack of support. The journal is to be subsidised by the American Association for the Advancement of Science, and by Prof. A. Graham Bell and the Hon. Gardiner G. Hubbard; and provided there be a liberal response to the circular from intending subscribers, the journal will be resuscitated before long.

PROF. BRUNCHORST has published an account of the laboratory and scientific appliances of the Marine Zoological Station at Bergen. Established in 1892, the station has always been kept open throughout the entire year, the fjords on the west coast of Norway remaining open throughout the winter, and the air temperature seldom falling much below the freezing-point.

THE following ode to Helmholtz appeared in *Punch* of last week, and seems to us so admirable that we reprint it :—

HELMHOLTZ.

WHAT matter titles? HELMHOLTZ is a name
That challenges, alone, the award of Fame!
When Emperors, Kings, Pretenders, shadows all,
Leave not a dust-trace on our whirling ball,
Thy work, oh grave-eyed searcher, shall endure,
Unmarred by faction, from low passion pure.
To bridge the gulf 'twixt matter-veil and mind
Perchance to mortals, dull-sensed, slow, purblind,
Is not permitted—yet; but patient, keen,
Thou on the shadowy track beyond the Seen,
Didst dog the elusive truth, and seek in sound
The secret of soul-mysteries profound,
Essential Order, Beauty's hidden law!
Marvels to strike more sluggish souls with awe,
Great seekers, lonely-souled, explore that track,
We welcome the wild wonders they bring back
From ventures stranger than an earthly Pole
Can furnish. Distant still that mental goal
To which great spirits strain; but when calm Fame
Sums its bold seekers, HELMHOLTZ, thy great name
Among the foremost shall eternal stand,
Science's pride, and glory of thy land.

FROM time to time paragraphs appear in the daily papers informing the public that a cure for consumption has been discovered. The last of these so-called discoveries has been heralded in the *Times* (September 14), where it is stated on the authority (?) of the Havas Agency, that a Genoese physician has been able to cure twenty-five out of twenty-seven hopeless cases of consumption by the subcutaneous injection of asses' blood. Strangely enough, the medical papers have remained silent, and we cannot help thinking that it would have been well had the *Times* not been so eager to advertise this mode of treatment before it had gone through the ordeal of medical criticism. The subcutaneous injection of serum of animals into phthisical patients has been extensively tried already and has failed, and it is not likely that asses' serum would have more therapeutic properties than that of dogs or goats, which has proved a failure. It is likely, moreover, that such a premature announcement will do a great deal of harm by raising the hopes of patients and their friends—hopes which are almost sure to be disappointed.

ACCORDING to news received by the Agent-General for Tasmania, the whale fishery industry of that colony, which for some years past has been in a feeble condition, has recently undergone a revival, whales having been frequently seen on the Tasmanian coasts within the last month or so.

THE third annual report of the Department of Agriculture of the Yorkshire College, Leeds, has reached us, and tells of a vast amount of work accomplished during the period under review,—1893–94. The courses of lectures for farmers, &c., and classes for elementary teachers and dairy teachers, and the travelling dairy schools, seem, on the whole, to have been well attended, and the committee has reason for the feeling of satisfaction to which it gives expression. A prospectus of the courses in agriculture for the session 1894–95 is now ready, and may be had of the secretary.

A SERIES of new boron compounds containing fluorine and alcohol radicles, derived from the interaction of boron fluoride and alcohols, are described by M. Gasselin in the September number of the *Annales de Chimie et de Physique*. The mono- and di-fluorine compounds derived from methyl and ethyl alcohol have been isolated in the pure state, and prove to be substances of great chemical activity, affording numerous interesting reactions. When boron trifluoride gas is passed into methyl or ethyl alcohol, strongly cooled by a freezing mixture, the gas is rapidly absorbed and the liquid becomes considerably heated. The reaction occurs in exactly equal molecular proportions, and upon subsequent distillation of the liquid product two main substances are eventually isolated. The first is the di-fluorine compound $\mathrm{B}\begin{smallmatrix}\diagup F_2\\ \diagdown OCH_3\end{smallmatrix}$ or $\mathrm{B}\begin{smallmatrix}\diagup F_2\\ \diagdown OC_2H_5\end{smallmatrix}$, while the second is a remarkable molecular compound of boron trifluoride with methyl or ethyl ether, $BF_3.(CH_3)_2O$ or $BF_3.(C_2H_5)_2O$. The reaction is quantitatively expressed by the following equation, in which R represents the alkyl radicle:

$$7\,BF_3 + 7\,ROH = \mathrm{B}\begin{smallmatrix}\diagup F_2\\ \diagdown OR\end{smallmatrix} + 3\,(BF_3\,.\,R_2O) + 2\,BF_4H + 2\,HF + B(OH)_3.$$

Di-fluor methyl borate $\mathrm{B}\begin{smallmatrix}\diagup F_2\\ \diagdown OCH_3\end{smallmatrix}$ distils over as a colourless liquid boiling at 80°. It solidifies in the receiver in the form of long crystals which melt at 41°·5. The analogous ethyl compound boils at 82°, and the crystals melt at 23°. The liquids fume strongly in the air, disseminating suffocating vapours. Water decomposes them with great energy, producing boric acid, fluoboric acid, and the free alcohol. They are insoluble in hydrocarbons, but dissolve with decomposition in alcohol. They are quite permanent in contact with metallic sodium, even under pressure at 100°. Sodium methylate or ethylate, however, react with great energy when brought in contact with them, and if equal molecular proportions are employed, the mono-fluorine compounds are produced.

$$\mathrm{B}\begin{smallmatrix}\diagup F_2\\ \diagdown OR\end{smallmatrix} + RONa = \mathrm{B}\begin{smallmatrix}\diagup F\\ \diagdown (OR)_2\end{smallmatrix} + NaF.$$

The methyl compound $\mathrm{B}\begin{smallmatrix}\diagup F\\ \diagdown (OCH_3)_2\end{smallmatrix}$ boils at 53°, and is a particularly mobile and strongly fuming liquid, which burns with a brilliant green flame, surrounded by a dense white cloud. The ethyl compound is a liquid of similar properties, which boils at 78°. Water decomposes both compounds with some violence and considerable evolution of heat. The molecular compounds of boron trifluoride with methyl and ethyl ether are fuming liquids boiling, respectively, at 126° and 123°, which are likewise energetically decomposed by water. They have been independently prepared by direct union of gaseous boron fluoride with gaseous methyl ether in the one case, and ordinary ethyl ether in the other. The union is instantaneous, and accompanied by considerable rise of temperature in each case. In the case of the formation of the methyl compound a dense cloud is produced the moment the constituent gases come in contact, and the sides of the vessel become covered by hot drops of liquid which rapidly coalesce to form a considerable bulk of the new substance.

THE additions to the Zoological Society's Gardens during the past week include a Sykes's Monkey (*Cercopithecus albigularis*, ♂) from East Africa, presented by Miss Marion E. Leitch; a Macaque Monkey (*Macacus cynomolgus*, ♀) from India, presented by Captain W. Townsend; two Alligators (*Alligator mississippiensis*) from the Mississippi, presented by Mr. L. Watson; two Giant Toads (*Bufo marinus*) from Brazil, presented by F. E. Blaaw, Esq.; twenty European Tree Frogs (*Hyla arborea*) European, presented by Mr. G. B. Coleman; an Ostrich (*Struthio camelus*, ♂) from Africa, a Greater Sulphur-crested Cockatoo (*Cacactua galerita*) from Australia, deposited; a Red-sided Eclectus (*Eclectus pectoralis*) from New Guinea, a Toco Toucan (*Rhamphastos toco*) from Guiana, a Diamond Snake *Morelia spilotes*) from New South Wales, received in exchange; an African Wild Ass) *Equus tæniopus*, ♂), an Axis Deer (*Cervus axis*, ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE ACCURACY OF ASTRONOMICAL OBSERVATIONS.—At the tenth general meeting of the Association Géodésique Internationale, Prof. Cornu read a paper on the necessity of introducing additional precautions in astronomical observations requiring great accuracy. The subject of the paper was suggested by the interpretation put upon observed variations of latitude. Prof. Cornu first remarked that, while carrying out their experiments on the earth's density some time ago, J. B. Baille and himself found that the constant of gravitation appeared to undergo an annual variation, being a little greater in spring than in autumn. A minute examination of the experimental conditions showed the investigators that this periodic anomaly was purely an apparent phenomenon, and that it was caused by an annual variation in the temperature of the room in which the apparatus was placed. It is impossible not to be struck with the analogy of these results and those of the variations of latitude. In both cases the period is approximately an annual one, and the maxima and minima occur in spring and autumn respectively. The question arises, therefore, whether astronomers have been careful to eliminate all the meteorological causes affecting their results, and whether their claims for marvellous accuracy are not, to some extent, exaggerated. Prof. Cornu has applied the reasoning of the physicist to astronomical observations and instruments. Beginning with the meridian circle, he points out that, on a divided circle one metre in diameter, one-tenth of a second of arc corresponds in round numbers to $\frac{1}{2000000}$ of a metre, or 0·0005 mm., that is, a semi-micron. But numerous metrological experiments have shown that, even under the most favourable conditions, a semi-micron is the limit of precision in differential linear measures on scales one metre long; and to attain this limit, it is necessary to put the scales side by side in a bath having a practically constant temperature. But the circles of meridian instruments are subjected to all sorts of variations, hence it seems affectation to assume that observations made with them are true to one-tenth of a second of arc; yet that assumption is made in the discussion of observations of latitudinal variations. When the spirit-level, the telescope, and the micrometer are criticised from this physical point of view, they are found wanting in the extreme accuracy usually ascribed to them. And, in addition to the innate defects in the parts of a meridian instrument, there are the temperature variations which almost baffle estimation. Several additional precautions should be taken to reduce this vitiating cause. In the first place, the distribution of temperature around and inside the instrument should be frequently determined, so that the proper corrections for refraction could be made. The conductivity of the parts of the instrument ought also to be increased, and made as nearly equal as possible, in order to reduce flexional and torsional effects produced by inequality of temperature. Another improvement would be to reduce the quantity of heat emitted by light-sources in observatories, and, finally, attempts should be made to give the air in the telescope tube the same temperature as that outside; so that systematic errors of refraction might be eliminated. Prof. Cornu thinks it is only by having recourse to precautions of this kind that definite results on the variability of latitudes can be obtained. In the actual state of the observations, he says, two purely physical objections exist against the reality of the phenomenon. They are:—(1) Is it certain that observations of latitude by the Talcott method are free from periodic annual errors due to meteorological influences, particularly to the variation of temperature during different seasons? (2) Can it be demonstrated that the astronomical data used in the preparation of the catalogues of the stars employed in these observations are also free from the errors referred to?

LIVERPOOL OBSERVATORY.—In a small pamphlet, published by order of the Mersey Docks and Harbour Board, we have placed before us some extracts from the report of the Director of the Observatory, Mr. W. E. Plummer, to the Marine Committee. The excessive briefness of the extracts in question curtail our remarks very considerably. The transit instrument, for time determinations, has been used on every possible occasion, and the normal clock has been maintained more accurately during cloudy weather by the adoption of separate clock errors and rates for the Bond and Molyneux clocks. The determination of the longitude (not *longtitude*, as is twice printed) of the Observatory has not yet been completed, as the repetition of the signals has not yet been effected, but the inquiry at present indicates a change from previous values, the Observatory acquiring a new position some 400 yards west of the old one. The latitude observations, by the method of transits of zenith stars over the prime vertical, although said to give this quantity with great exactness, are likely to be disappointing when used for discussing the small variation in latitude, for the instrument and its mounting have shown signs of a slight instability. Besides the observations of comets, stellar parallaxes, by the method of chronographic record of meridian transits, have been investigated. The meteorological observations have, as usual, been continued, and the present report contains numerous tables of the results obtained.

THE VARIABLE R LYRÆ.—The variability of this star was first pointed out by Baxendall in 1856, and its period was stated to be one of forty-eight days. At a somewhat later date Schonfeld, from a few more observations, deduced a forty-six-day period. Generally the observations have been somewhat scarce, but more recently their number has been considerably increased, Yendell, Sawyer, Plassmann, and Knopf having been chiefly responsible for these. That the time had arrived when a more thorough investigation of this "lichtwechsel" might be attempted, is the opinion of Herr A. Pannekoek, and the details of his work will be found in *Astronomische Nachrichten*, No. 3252. In the examination of the observations it was soon found out that the period did not appear to remain constant. In the earlier epochs, commencing in 1887 and continuing up to the past year, the numbers indicated clearly a period of about forty-six days, but recently they have somewhat increased. In general, also, it has been noticed that the time for the increase in the light curve is shorter than that for the decrease. Herr Pannekoek concludes, however, from his work, that R Lyræ does not vary its brightness so irregularly as is supposed. He suggests that the observed apparent deviations recorded can be attributed to special errors of observation. He is of opinion, however, that perhaps when many more observations are at hand, we shall be in a far better position to investigate the subject more thoroughly, and to state more definitely through what amplitudes these light variations swing.

THE CLEANING OF OBJECT-GLASSES.—Owners of telescopes (refractors) will no doubt be glad to have a few words of practical advice, from one who can speak from long experience, with regard to the cleaning of object-glasses. The advice in question is extracted from an article, by Mr. Brashear, in the September number of *Popular Astronomy*, and we do not hesitate in helping to distribute it. The reader may be rather surprised to hear that the use of fine chamois-skin, tissue-paper, or an old soft silk handkerchief, or "any other such material to wipe lenses, as is usually advised," are not advocated. The reason for this is, not that the materials themselves do the mischief, but that the chief enemy to an object-glass, "dust particles," and these most likely of a siliceous nature, must not on any account be rubbed on the glassy surface. The receipt, in a few words, may be summed up as follows:—If the lenses be dirty or dusty, a tuft of cotton or a camel's-hair brush may be at first applied, but pains should be taken that no pressure be given to either. For further cleaning, a *wooden* bowl, previously washed out with soap and water, should be filled with clean water of approximately the same temperature as the objective. A little ammonia (quantity, a teaspoonful to half a pail of water) should be added to the water. "Cheese-cloth" is strongly recommended as a means of applying the soap to the glass; but this, first, should be "thoroughly washed with soap and water," and thrown away when done with. Plenty of water must always be used. A third or fourth cloth should be used to wipe the objective dry. "Vigorous rubbing will do no harm if the surfaces have no abrading material on them, and I have yet to injure a glass cleaned in this way." If the objective be not taken from the cell, the camel's-hair brush and the soap-and-water process can be still used, and the work finished with a dry cheese-cloth. Mr. Brashear gives good reasons for taking the lenses apart from time to time, and giving them a thorough clean; and he adds that everyone who owns and uses a telescope should be so familiar with his objective that he can take it apart and put it together just as well as the maker of it. In moist climates particularly this should be done frequently.

ON THE MAGNITUDE OF THE SOLAR SYSTEM.[1]

NATURE may be studied in two widely different ways. On the one hand, we may employ a powerful microscope which will render visible the minutest forms and limit our field of view to an infinitesimal fraction of an inch situated within a foot of our own noses; or, on the other hand, we may occupy some commanding position, and from thence, aided perhaps by a telescope, we may obtain a comprehensive view of an extensive region. The first method is that of the specialist, the second is that of the philosopher, but both are necessary for an adequate understanding of nature. The one has brought us knowledge wherewith to defend ourselves against bacteria and microbes which are among the mostly deadly enemies of mankind, and the other has made us acquainted with the great laws of matter and force upon which rests the whole fabric of science. All nature is one, but for convenience of classification we have divided our knowledge into a number of sciences which we usually regard as quite distinct from each other. Along certain lines, or, more properly, in certain regions, these sciences necessarily abut on each other, and just there lies the weakness of the specialist. He is like a wayfarer who always finds obstacles in crossing the boundaries between two countries, while to the traveller who gazes over them from a commanding eminence the case is quite different. If the boundary is an ocean shore, there is no mistaking it; if a broad river or a chain of mountains, it is still distinct; but if only a line of posts traced over hill and dale, then it becomes lost in the natural features of the landscape, and the essential unity of the whole region is apparent. In that case the border-land is wholly a human conception of which nature takes no cognisance, and so it is with the scientific border-land to which I propose to invite your attention this evening.

To the popular mind there are no two sciences further apart than astronomy and geology. The one treats of the structure and mineral constitution of our earth, the causes of its physical features and its history; while the other treats of the celestial bodies, their magnitudes, motions, distances, periods of revolution, eclipses, order, and of the causes of their various phenomena. And yet many, perhaps I may even say most, of the apparent motions of the heavenly bodies are merely reflections of the motions of the earth, and in studying them we are really studying it. Furthermore, precision, mutation, and the phenomena of the tides depend largely upon the internal structure of the earth, and there astronomy and geology merge into each other. Nevertheless the methods of the two sciences are widely different, most astronomical problems being discussed quantitatively by means of rigid mathematical formulæ, while in the vast majority of cases the geological ones are discussed only qualitatively, each author contenting himself with a mere statement of what he thinks. With precise data the methods of astronomy lead to very exact results, for mathematics is a mill which grinds exceeding fine; but after all, what comes out of a mill depends wholly upon what is put into it, and if the data are uncertain, as is the case in most cosmological problems, there is little to choose between the mathematics of the astronomer and the guesses of the geologist.

If we examine the addresses delivered by former presidents of this Association, and of the sister—perhaps it would be nearer the truth to say the parent Association on the other side of the Atlantic—we shall find that they have generally dealt either with the recent advances in some broad field of science, or else with the development of some special subject. This evening I propose to adopt the latter course, and I shall invite your attention to the present condition of our knowledge respecting the magnitude of the solar system; but in so doing, it will be necessary to introduce some considerations derived from laboratory experiments upon the luminiferous ether, others derived from experiments from ponderable matter, and still others relating both to the surface phenomena and to the internal structure of the earth, and thus we shall deal largely with the border-land where astronomy, physics, and geology merge into each other.

The relative distances of the various bodies which compose the solar system can be determined to a considerable degree of approximation with very crude instruments as soon as the true plan of the system becomes known, and that plan was taught by Pythagoras more than five hundred years before Christ. It must have been known to the Egyptians and Chaldeans still earlier, if Pythagoras really acquired his knowledge of astronomy from them, as is affirmed by some of the ancient writers, but on that point there is no certainty. In public Pythagoras seemingly accepted the current belief of his time, which made the earth the centre of the universe, but to his own chosen disciples he communicated the true doctrine that the sun occupies the centre of the solar system, and that the earth is only one of the planets revolving around it. Like all the world's greatest sages, he seems to have taught only orally. A century elapsed before his doctrines were reduced to writing by Philolaus of Crotona, and it was still later before they were taught in public for the first time by Hicetas, or, as he is sometimes called, Nicetas, of Syracuse. Then the familiar cry of impiety was raised, and the Pythagorean system was eventually suppressed by that now called the Ptolemaic, which held the field until it was overthrown by Copernicus almost two thousand years later. Pliny tells us that Pythagoras believed the distances to the sun and moon to be respectively 252,000 and 12,600 stadia, or taking the stadium at 625 feet, 29,837 and 1492 English miles; but there is no record of the method by which these numbers were ascertained.

After the relative distances of the various planets are known, it only remains to determine the scale of the system, for which purpose the distance between any two planets suffices. We know little about the early history of the subject, but it is clear that the primitive astronomers must have found the quantities to be measured too small for detection with their instruments, and even in modern times the problem has proved to be an extremely difficult one. Aristarcus of Samos, who flourished about 270 B.C., seems to have been the first to attack it in a scientific manner.

Stated in modern language, his reasoning was that when the moon is exactly half full, the earth and sun as seen from its centre must make a right angle with each other, and by measuring the angle between the sun and moon, as seen from the earth at that instant, all the angles of the triangle joining the earth, sun, and moon would become known, and thus the ratio of the distance of the sun to the distance of the moon would be determined. Although perfectly correct in theory, the difficulty of deciding visually upon the exact instant when the moon is half full is so great that it cannot be accurately done even with the most powerful telescopes. Of course, Aristarcus had no telescope, and he does not explain how he effected the observation, but his conclusion was that at the instant in question the distance between the centres of the sun and moon, as seen from the earth, is less than a right angle by 1/30 part of the same. We should now express this by saying that the angle is 87°; but Aristarcus knew nothing of trigonometry, and in order to solve his triangle he had recourse to an ingenious but long and cumbersome geometrical process which has come down to us, and affords conclusive proof of the condition of Greek mathematics at that time. His conclusion was that the sun is nineteen times further from the earth than the moon, and if we combine that result with the modern value of the moon's parallax, viz. 3422·38 seconds, we obtain for the solar parallax 180 seconds, which is more than twenty times too great.

The only other method of determining the solar parallax known to the ancients was that devised by Hipparchus about 150 B.C. It was based on measuring the rate of decrease of the diameter of the earth's shadow cone by noting the duration of lunar eclipses, and as the result deduced from it happened to be nearly the same as that found by Aristarcus, substantially his value of the parallax remained in vogue for nearly two thousand years, and the discovery of the telescope was required to reveal its erroneous character. Doubtless this persistency was due to the extreme minuteness of the true parallax, which we now know is far too small to have been visible upon the ancient instruments, and thus the supposed measures of it were really nothing but measures of their inaccuracy.

The telescope was first pointed to the heavens by Galileo in 1609, but it needed a micrometer to convert it into an accurate measuring instrument, and that did not come into being until 1639, when it was invented by William Gascoigne. After his death in 1644, his original instrument passed to Richard Towneley, who attached it to a fourteen-foot telescope at his residence in Townley, Lancashire, England, where it was used by Flamsteed in observing the diurnal parallax of Mars during its oppo-

[1] Address delivered before the American Association for the Advancement of Science, at its Brooklyn Meeting, August 16, by the retiring President, William Harkness.

sition in 1672. A description of Gascoigne's micrometer was published in the "Philosophical Transactions" in 1667, and a little before that a similar instrument had been invented by Auzout in France; but observatories were fewer then than now, and so far as I know J. D. Cassini was the only person beside Flamsteed who attempted to determine the solar parallax from that opposition of Mars. Foreseeing the importance of the opportunity, he had Richer despatched to Cayenne some months previously, and when the opposition came he effected two determinations of the parallax; one being by the diurnal method, from his own observations in Paris, and the other by the meridian method, from observations in France by himself, Romer and Picard, combined with those of Richer at Cayenne. This was the transition from the ancient instruments with open sights to telescopes armed with micrometers, and the result must have been little short of stunning to the seventeenth century astronomers, for it caused the hoary and gigantic parallax of about 180 seconds to shrink incontinently to ten seconds, and thus expanded their conception of the solar system to something like its true dimensions. More than fifty years previously Kepler had argued from his ideas of the celestial harmonies that the solar parallax could not exceed sixty seconds, and a little later Horrocks had shown on more scientific grounds that it was probably as small as fourteen seconds, but the final death-blow to the ancient values, ranging as high as two or three minutes, came from these observations of Mars by Flamsteed, Cassini, and Richer.

Of course the results obtained in 1672 produced a keen desire on the part of astronomers for further evidence respecting the true value of the parallax, and as Mars comes into a favourable position for such investigations only at intervals of about sixteen years, they had recourse to observations of Mercury and Venus. In 1677 Halley observed the diurnal parallax of Mercury, and also a transit of that planet across the sun's disc at St. Helena, and in 1681 J. D. Cassini and Picard observed Venus when she was on the same parallel with the sun; but although the observations of Venus gave better results than those of Mercury, neither of them was conclusive, and we now know that such methods are inaccurate, even with the powerful instruments of the present day. Nevertheless, Halley's attempt by means of the transit of Mercury ultimately bore fruit in the shape of his celebrated paper of 1716, wherein he showed the peculiar advantages of transits of Venus for determining the solar parallax. The idea of utilising such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks; but Halley was the first to work it out completely, and long after his death his paper was mainly instrumental in inducing the Governments of Europe to undertake the observations of the transits of Venus in 1761 and 1769, from which our first accurate knowledge of the sun's distance was obtained.

Those who are not familiar with practical astronomy may wonder why the solar parallax can be got from Mars and Venus, and not from Mercury, or the sun itself. The explanation depends on two facts—firstly, the nearest approach of these bodies to the earth is for Mars 33,874,000 miles, for Venus 23,654,000 miles, for Mercury 47,935,000 miles, and for the sun 91,239,000 miles. Consequently, for us, Mars and Venus have very much larger parallaxes than Mercury or the sun, and of course the larger the parallax the easier it is to measure. Secondly, even the largest of these parallaxes must be determined within far less than one-tenth of a second of the truth; and while that degree of accuracy is possible in measuring short arcs, it is quite unattainable in long ones. Hence one of the most essential conditions for the successful measurement of parallaxes is that we shall be able to compare the place of the near body with that of a more distant one situated in the same region of the sky. In the case of Mars, that can always be done by making use of a neighbouring star, but when Venus is near the earth she is also so close to the sun that stars are not available, and consequently her parallax can be satisfactorily measured only when her position can be accurately referred to that of the sun; or, in other words, only during her transits across the sun's disk. But even when the two bodies to be compared are sufficiently near each other, we are still embarrassed by the fact that it is more difficult to measure the distance between the limb of a planet and a star or the limb of the sun, than it is to measure the distance between two stars; and since the discovery of so many asteroids, that circumstance has led to their use for determinations of the solar parallax. Some of these bodies approach within 75,230,000 miles of the earth's orbit, and as they look precisely like stars, the increased accuracy of pointing on them fully makes up for their greater distance, as compared with Mars or Venus.

After the Copernican system of the world and the Newtonian theory of gravitation were accepted, it soon became evident that trigonometrical measurements of the solar parallax might be supplemented by determinations based on the theory of gravitation, and the first attempts in that direction were made by Machin in 1729 and T. Mayer in 1753. The measurement of the velocity of light between points on the earth's surface, first effected by Fizeau in 1849, opened up still other possibilities, and thus for determining the solar parallax we now have at our command no less than three entirely distinct classes of methods, which are known respectively as the trigonometrical, the gravitational, and the photo-tachymetrical. We have already given a summary sketch of the trigonometrical methods, as applied by the ancient astronomers to the dichotomy and shadow cone of the moon, and by the moderns to Venus, Mars, and the asteroids, and we shall next glance briefly at the gravitational and photo-tachymetrical methods.

The gravitational results which enter directly or indirectly into the solar parallax are six in number, to wit: first, the relation of the moon's mass to the tides; second, the relation of the moon's mass and parallax to the force of gravity at the earth's surface; third, the relation of the solar parallax to the masses of the earth and moon; fourth, the relation of the solar and lunar parallaxes to the moon's mass and parallactic inequality; fifth, the relation of the solar and lunar parallaxes to the moon's mass and the earth's lunar inequality; sixth, the relation of the constants of nutation and precession to the moon's parallax.

Respecting the first of these relations, it is to be remarked that the tide-producing forces are the attraction of the sun and moon upon the waters of the ocean, and from the ratio of these attractions the moon's mass can readily be determined. But unfortunately the ratio of the solar tides to the lunar tides is affected both by the depth of the sea and by the character of the channels through which the water flows, and for that reason the observed ratio of these tides requires multiplication by a correcting factor in order to convert it into the ratio of the forces. The matter is further complicated by this correcting factor varying from port to port, and in order to get satisfactory results long series of observations are necessary. The labour of deriving the moon's mass in this way was formerly so great that for more than half a century La Place's determination from the tides at Brest remained unique, but the recent application of harmonic analysis to the data supplied by self-registering tide gauges is likely to yield abundant results in the near future.

Our second gravitational relation, viz. that connecting the moon's mass and parallax with the force of gravity at the earth's surface, affords an indirect method of determining the moon's parallax with very great accuracy if the computation is carefully made, and with a fair approximation to the truth even when the data are exceedingly crude. To illustrate this, let us see what could be done with a railroad transit such as is commonly used by surveyors, a steel tape, and a fairly good watch. Neglecting small corrections due to the flattening of the earth, the centrifugal force at its surface, the eccentricity of its orbit, and the mass of the moon, the law of gravitation shows that if we multiply together the length of the seconds pendulum, the square of the radius of the earth, and the square of the length of the sidereal month, divide the product by four, and take the cube root of the quotient, the result will be the distance from the earth to the moon. To find the length of the seconds pendulum we would rate the watch by means of the railroad transit, and then making a pendulum out of a spherical leaden bullet suspended by a fine thread, we would adjust the length of the thread until the pendulum made exactly 300 vibrations in five minutes by the watch. Then, supposing the experiment to be made here, or in New York city, we would find that the distance from the point of suspension of the thread to the centre of the bullet was about 39 and 1/8 inches, and dividing that by the number of inches in a mile, viz. 63,360, we would have for the length of the seconds pendulum one-sixteen hundred and twentieth of a mile. The next step would be to ascertain the radius of the earth, and the quickest way of doing so would probably be, first, to determine the latitude of some point in New York city by means of the railroad transit; next, to run

a traverse survey along the old Post Road from New York to Albany, and finally, to determine the latitude of some point in Albany. The traverse survey should surely be correct to one part in three hundred, and as the distance between the two cities is about two degrees, the difference of latitude might be determined to about the same percentage of accuracy. In that way we would find the length of two degrees of latitude to be about 138 miles, whence the earth's radius would be 3953 miles. It would then only remain to observe the time occupied by the moon in making a sidereal revolution around the earth, or, in other words, the time which she occupies in moving from any given star back to the same star again. By noting that to within one-quarter of her own diameter we should soon find that the time of a revolution is about 27·32 days, and multiplying that by the number of seconds in a day, viz. 86,400, we would have for the length of the sidereal month 2,360,000 seconds. With these data the computation would stand as follows:—The radius of the earth, 3953 miles, multiplied by the length of a sidereal month, 2,360,000 seconds, and the product squared, gives 87,060,000,000,000,000,000. Multiplying that by one-fourth of the length of the seconds pendulum, viz. 1/6480 of a mile, and extracting the cube root of the product, we would get 237,700 miles for the distance from the earth to the moon, which is only about 850 miles less than the truth, and certainly a remarkable result considering the crudeness of the instruments by which it might be obtained. Nevertheless, when all the conditions are rigorously taken into account, these data are to be regarded as determining the relation between the moon's mass and parallax rather than the parallax itself.

Our third gravitational relation, to wit, that existing between the solar parallax, the solar attractive force and the masses of the earth and moon, is analogous to the relation existing between the moon's mass and parallax and the force of gravity at the earth's surface, but it cannot be applied in exactly the same way, on account of our inability to swing a pendulum on the sun. We are therefore compelled to adopt some other method of determining the sun's attractive force, and the most available is that which consists in observing the perturbative action of the earth and moon upon our nearest planetary neighbours, Venus and Mars. From this action the law of gravitation enables us to determine the ratio of the sun's mass to the combined masses of the earth and moon, and then the relation in question furnishes a means of comparing the masses so found with trigonometrical determinations of the solar parallax. Thus it appears that notwithstanding necessary differences in the methods of procedure, the analogy between the second and third gravitational relations holds not only with respect to their theoretical basis, but also in their practical application, the one being used to determine the relation between the mass of the moon and its distance from the earth, and the other to determine the relation between the combined masses of the earth and noon and their distance from the sun.

Our fourth gravitational relation deals with the connection between the solar parallax, the lunar parallax, the moon's mass and the moon's parallactic inequality. The important quantities are here the solar parallax and the moon's parallactic inequality, and although the derivation of the complete expression for the connection between them is a little complicated, there is no difficulty in getting a general notion of the forces involved. As the moon moves around the earth she is alternately without and within the earth's orbit. When she is without, the sun's attraction on her acts with that of the earth; when she is within, the two attractions act in opposite directions. Thus in effect the centrifugal force holding the moon to the earth is alternately increased and diminished, with the result of elongating the moon's orbit towards the sun and compressing it on the opposite side. As the variation of the centrifugal force is not great, the change of the form of the orbit is small, nevertheless the summation of the minute alterations thereby produced in the moon's orbital velocity suffices to put her sometimes ahead, and sometimes behind her mean place to an extent which oscillates from a maximum to a minimum as the earth passes from perihelion to aphelion, and averages about 125 seconds of arc. This perturbation of the moon's node is known as the parallactic inequality because it depends on the earth's distance from the sun, and can therefore be expressed in terms of the solar parallax. Conversely, the solar parallax can be deduced from the observed value of the parallactic inequality, but unfortunately there are great practical difficulties in making the requisite observations with a sufficient degree of accuracy. Notwithstanding the ever-recurring talk about the advantages to be obtained by observing a small well-defined crater instead of the moon's limb, astronomers have hitherto found it impracticable to use anything but the limb, and the disadvantage of doing so as compared with observing a star is still further increased by the circumstances that in general only one limb can be seen at a time, the other being shrouded in darkness. If both limbs could always be observed, we should then have a uniform system of data for determining the place of the centre, but under existing circumstances we are compelled to make our observations half upon one limb and half upon the other, and thus they involve all the systematic errors which may arise from the conditions under which these limbs are observed, and all the uncertainty which attaches to irradiation, personal equation, and our defective knowledge of the moon's semi-diameter.

Our fifth gravitational relation is that which exists between the solar parallax, the lunar parallax, the moon's mass, and the earth's lunar inequality. Strictly speaking, the moon does not revolve around the earth's centre, but both bodies revolve around the common centre of gravity of the two. In consequence of that an irregularity arises in the earth's orbital velocity around the sun, the common centre of gravity moving in accordance with the laws of elliptic motion, while the earth, on account of its revolution around that centre, undergoes an alternate acceleration and retardation which has for its period a lunar month, and is called the lunar inequality of the earth's motion. We perceive this inequality as an oscillation superposed on the elliptic motion of the sun, and its semi-amplitude is a measure of the angle subtended at the sun by the interval between the centre of the earth and the common centre of gravity of the earth and moon. Just as an astronomer on the moon might use the radius of her orbit around the earth as a base for measuring her distance from the sun, so we may use this interval for the same purpose. We find its length in miles from the equatorial semi-diameter of the earth, the moon's parallax and the moon's mass, and thus we have all the data for determining the solar parallax from the inequality in question. In view of the great difficulty which has been experienced in measuring the solar parallax itself, it may be asked why we should attempt to deal with the parallactic inequality which is about twenty-six per cent. smaller? The answer is, because the latter is derived from differences of the sun's right ascension which are furnished by the principal observatories in vast numbers, and should give very accurate results on account of their being made by methods which insure freedom from constant errors. Nevertheless, the sun is not so well adapted for precise observations as the stars, and Dr. Gill has recently found that heliometer measurements upon asteroids which approach very near to the earth yield values of the parallactic inequality superior to those obtained from right ascensions of the sun.

Our sixth gravitational relation is that which exists between the moon's parallax and the constants of precession and nutation. Every particle of the earth is attracted both by the sun and by the moon, but in consequence of the polar flattening the resultant of these attractions passes a little to one side of the earth's centre of gravity. Thus a couple is set up, which, by its action upon the rotating earth, causes the axis thereof to describe a surface which may be called a fluted cone, with its apex at the earth's centre. A top spinning with its axis inclined describes a similar cone, except that the flutings are absent, and the apex is at the point upon which the spinning occurs. For convenience of computation we resolve this action into two components, and we name that which produces the cone the luni-solar precession, and that which produces the flutings the nutation. In this phenomenon the part played by the sun is comparatively small, and by eliminating it we obtain a relation between the luni-solar precession, the nutation and the moon's parallax, which can be used to verify and correct the observed values of these quantities.

In the preceding paragraph we have seen that the relation between the quantities there considered depends largely upon the flattening of the earth, and thus we are led to inquire how and with what degree of accuracy that is determined. There are five methods, viz. one geodetic, one gravitational, and three astronomical. The geodetic method depends upon measurements of the length of a degree on various parts of the earth's surface, and with the data hitherto accumulated it has proved quite unsatisfactory. The gravitational method consists in de-

termining the length of the seconds pendulum over as great a range of latitude as possible, and deducing therefrom the ratio of the earth's polar and equatorial semi-diameters by means of Clairaut's theorem. The pendulum experiments show that the earth's crust is less dense on mountain plateaux than at the sea coast, and thus for the first time we are brought into contact with geological considerations. The first astronomical method consists in observing the moon's parallax from various points on the earth's surface, and as these parallaxes are nothing else than the angular semi-diameter of the earth at the respective points as seen from the moon, they afford a direct measure of the flattening. The second and third astronomical methods are based upon certain perturbations of the moon which depend upon the figure of the earth, and should give extremely accurate results, but unfortunately very great difficulties oppose themselves to the exact measurement of the perturbations. There is also an astronomico-geological method which cannot yet be regarded as conclusive, on account of our lack of knowledge respecting the law of density which prevails in the interior of the earth. It is based upon the fact that a certain function of the earth's moments of inertia can be determined from the observed values of the coefficients of precession and nutation, and could also be determined from the figure and dimensions of the earth if we knew the exact distribution of matter in its interior. Our present knowledge on that subject is limited to a superficial layer not more than ten miles thick, but it is usual to assume that the deeper matter is distributed according to La Grange's law, and then by writing the function in question in a form which leaves the flattening indeterminate, and equating the expression so found to the value given by the precession and nutation, we readily obtain the flattening. As yet these six methods do not give consistent results, and so long as serious discrepancies remain between them, there can be no security that we have arrived at the truth.

It should be remarked that in order to compute the function of the earth's moments of inertia, which we have just been considering, we require not only the figure and dimensions of the earth and the law of distribution of density in its interior, but also its mean and surface densities. The experiments for determining the mean density have consisted in comparing the earth's attraction with the attraction either of a mountain, or of a known thickness of the earth's crust, or of a known mass of metal. In the case of mountains, the comparisons have been made with plumb-lines and pendulum; in the case of known layers of the earth's crust, they have been made by swinging pendulums at the surface and down in mines; and in the case of known masses, they have been made with torsion balances, fine chemical balances, and pendulums. The surface density results from a study of the materials composing the earth's crust; but notwithstanding the apparent simplicity of that process, it is doubtful if we have yet attained as accurate a result as in the case of the mean density.

Before quitting this part of our subject, it is important to point out that the luni-solar precession cannot be directly observed, but must be derived from the general precession. The former of these quantities depends only upon the action of the sun and moon, while the latter is affected in addition by the action of all the planets, and to ascertain what that is we must determine their masses. The methods of doing so fall into two great classes, according as the planets dealt with have or have not satellites. The most favourable case is that in which one or more satellites are present, because the mass of the primary follows immediately from their distances and revolution times, but even then there is a difficulty in the way of obtaining very exact results. By extending the observations over sufficiently long periods the revolution times can be ascertained with any desired degree of accuracy, but all measurements of the distance of a satellite from its primary are affected by personal equation, which we cannot be sure of completely eliminating, and thus a considerable margin of uncertainty is brought into the masses. In the cases of Mercury and Venus, which have no satellites, and to a certain extent in the case of the earth also, the only available way of ascertaining the masses is from the perturbations produced by the action of the various planets on each other. These perturbations are of two kinds, periodic and secular. When sufficient data have been accumulated for the exact determination of the secular perturbations, they will give the best results, but as yet it remains advantageous to employ the periodic perturbations also.

Passing now to the photo-tachymetrical methods, we have first to glance briefly at the mechanical appliances by which the tremendous velocity of light has been successfully measured. They are of the simplest possible character, and are based either upon a toothed-wheel, or upon a revolving mirror.

The toothed-wheel method was first used by Fizeau in 1849. To understand its operation, imagine a gun-barrel with a toothed-wheel revolving at right angles to its muzzle in such a way that the barrel is alternately closed and opened as the teeth and the spaces between them pass before it. Then, with the wheel in rapid motion, at the instant when a space is opposite the muzzle, let a ball be fired. It will pass out freely, and after traversing a certain distance, let it strike an elastic cushion and be reflected back upon its own path. When it reaches the wheel, if it hits a space it will return into the gun-barrel, but if it hits a tooth it will be stopped. Examining the matter a little more closely, we see that as the ball requires a certain time to go and return, if during that time the wheel moves through an odd multiple of the angle between a space and a tooth the ball will be stopped, while if it moves through an even multiple of that angle the ball will return into the barrel. Now imagine the gun-barrel, the ball, and the elastic cushion to be replaced respectively by a telescope, a light wave, and a mirror. Then if the wheel be moved at such a speed that the returning light wave struck against the tooth following the space through which it issued, to an eye looking into the telescope all would be darkness. If the wheel moved a little faster and the returning light wave passed through the space succeeding that through which it issued, the eye at the telescope would perceive a flash of light; and if the speed was continuously increased, a continual succession of eclipses and illuminations would follow each other according as the returning light was stopped against a tooth, or passed through a space further and further behind that through which it issued. Under these conditions the time occupied by the light in traversing the space from the wheel to the mirror and back again would evidently be the same as the time required by the wheel to revolve through the angle between the space through which the light issued and that through which it returned, and thus the velocity of light would become known from the distance between the telescope and the mirror together with the speed of the wheel. Of course the longer the distance traversed, and the greater the velocity of the wheel, the more accurate would be the result.

The revolving mirror method was first used by Foucault in 1862. Conceive the toothed-wheel of Fizeau's apparatus to be replaced by a mirror attached to a vertical axis, and capable of being put into rapid rotation. Then it will be possible so to arrange the apparatus that light issuing from the telescope shall strike the movable mirror and be reflected to the distant mirror, whence it will be returned to the movable mirror again, and being thrown back into the telescope will appear as a star in the centre of the field of view. That adjustment being made, if the mirror were caused to revolve at a speed of some hundred turns per second, it would move through an appreciable angle while the light was passing from it to the distant mirror and back again, and in accordance with the laws of reflection, the star in the field of the telescope would move from the centre by twice the angle through which the mirror had turned. Thus the deviation of the star from the centre of the field would measure the angle through which the mirror turned during the time occupied by light in passing twice over the interval between the fixed and revolving mirrors, and from the magnitude of that angle together with the known speed of the mirror, the velocity of the light could be calculated.

In applying either of these methods the resulting velocity is that of light when traversing the earth's atmosphere, but what we want is its velocity in space which we suppose to be destitute of ponderable material, and in order to obtain that the velocity in the atmosphere must be multiplied by the refractive index of air. The corrected velocity so obtained can then be used to find the solar parallax, either from the time required by light to traverse the semi-diameter of the earth's orbit, or from the ratio of the velocity of light to the orbital velocity of the earth.

Any periodic correction which occurs in computing the place of a heavenly body, or the time of a celestial phenomena, is called by astronomers an equation, and as the time required by light to traverse the semi-diameter of the earth's orbit first presented itself in the guise of a correction to the computed times of the eclipses of Jupiter's satellites, it has received the name of the light equation. The earth's orbit being interior to

that of Jupiter, and both having the sun for their centre, it is evident that the distance between the two planets must vary from the sun to the difference of the radii of their respective orbits, and the time required by light to travel from one planet to the other must vary proportionately. Consequently, if the observed times of the eclipses of Jupiter's satellites are compared with the times computed upon the assumption that the two planets are always separated by their mean distance, it will be found that the eclipses occur too early when the earth is at less than its mean distance from Jupiter, and too late when it is further off, and from large numbers of such observations the value of the light equation has been deduced.

The combination of the motion of light through our atmosphere with the orbital motion of the earth gives rise to the annual aberration, all the phases of which are computed from its maximum value, commonly called the constant of aberration. There is also a diurnal aberration due to the rotation of the earth on its axis, but that is quite small, and does not concern us this evening. When aberration was discovered the corpuscular theory of light was in vogue, and it offered a charmingly simple explanation of the whole phenomenon. The hypothetical light corpuscles impinging upon the earth were thought to behave precisely like the drops in a shower of rain, and you all know that their apparent direction is affected by any motion on the part of the observer. In a calm day, when the drops are falling perpendicularly, a man standing still holds his umbrella directly over his head, but as soon as he begins to move forward he inclines his umbrella in the same direction, and the more rapidly he moves the greater must be its inclination in order to meet the descending shower. Similarly the apparent direction of oncoming light corpuscles would be affected by the orbital motion of the earth, so that in effect it would always be the resultant arising from combining the motion of the light with a motion equal and opposite to that of the earth. But since the falsity of the corpuscular theory has been proved that explanation is no longer tenable, and as yet we have not been able to replace it with anything equally satisfactory based on the now universally accepted undulatory theory. In accordance with the latter theory we must conceive the earth as ploughing its way through the ether, and the point which has hitherto baffled us is whether or not in so doing it produces any disturbance of the ether which affects the aberration. In our present ignorance on that point we can only say that the aberration constant is certainly very nearly equal to the ratio of the earth's orbital velocity to the velocity of light, but we cannot affirm that it is rigorously so.

The luminiferous ether was invented to account for the phenomena of light, and for two hundred years it was not suspected to have any other function. The emission theory postulated only the corpuscles which constitute light itself, but the undulatory theory fills all space with an imponderable substance possessing properties even more remarkable than those of ordinary matter, and to some of the acutest intellects the magnitude of this idea has proved an almost insuperable objection against the whole theory. So late as 1862 Sir David Brewster, who had gained a world-wide reputation by his optical researches, expressed himself as staggered by the notion of filling all space with some substance merely to enable a little twinkling star to send its light to us; but not long after Clerk Maxwell removed that difficulty by a discovery coextensive with the undulatory theory itself. Since 1845, when Faraday first performed his celebrated experiment of magnetising a ray of light, the idea that electricity is a phenomenon of the ether had been steadily growing, until at last Maxwell perceived that if such were the fact the rate of propagation of an electromagnetic wave must be the same as the velocity of light. At that time no one knew how to generate such waves, but Maxwell's theory showed him that their velocity must be equal to the number of electric units of quantity in the electromagnetic unit, and careful experiments soon proved that that is the velocity of light. Thus it was put almost beyond the possibility of doubt that the ether gives rise to the phenomena of electricity and magnetism, as well as to those of light, and perhaps it may even be concerned in the production of gravitation itself. What could be apparently more remote than these electric quantities and the solar parallax? And yet we have here a relation between them, but we make no use of it, because as yet the same relation can be far more accurately determined from experiments upon the velocity of light.

Now let us recall the quantities and methods of observation which we have found to be involved either directly or indirectly with the solar parallax. They are the solar parallax, obtained from transits of Venus, oppositions of Mars, and oppositions of certain asteroids; the lunar parallax, found both directly, and from measurements of the force of gravity at the earth's surface; the constants of precision, nutation, and aberration, obtained from observations of the stars; the parallactic inequality of the moon; the lunar inequality of the earth, usually obtained from observations of the sun, but recently found from heliometer observations of certain asteroids; the mass of the earth, found from the solar parallax, and also from the periodic and secular perturbations of Venus and Mars; the mass of the moon, found from the lunar inequality of the earth, and also from the ratio of the solar and lunar components of the ocean tides; the masses of all the planets, obtained from observations of their satellites whenever possible, and when no satellites exist, then from observations of their mutual perturbations both periodic and secular; the velocity of light, obtained from experiments with revolving mirrors and toothed wheels, together with laboratory determinations of the index of refraction of atmospheric air; the light equation, obtained from observations of the ellipses of Jupiter's satellites; the figure of the earth, obtained from geodetic triangulations, measurements of the length of the seconds pendulum in various latitudes, and observations of certain perturbations of the moon; the mean density of the earth, obtained from measurements of the attractions of mountains, from pendulum experiments in mines, and from experiments on the attraction of known masses of matter made either with torsion balances or with the most delicate chemical balances; the surface density of the earth, obtained from geological examinations of the surface strata; and lastly, the law of distribution of density in the interior of the earth, which in the present state of geological knowledge we can do little more than guess at.

Here then we have a large group of astronomical, geodetic, geological and physical quantities which must all be considered in finding the solar parallax, and which are all so entangled with each other that no one of them can be varied without affecting all the rest. It is therefore impossible to make an accurate determination of any one of them apart from the remainder of the group, and thus we are driven to the conclusion that they must all be determined simultaneously. Such has not been the practice of astronomers in the past, but it is the method to which they must inevitably resort in the future. A cursory glance at an analogous problem occurring in geodesy may be instructive. When a country is covered with a net of triangles it is always found that the observed angles are subject to a certain amount of error, and a century ago it was the habit to correct the angles in each triangle without much regard to the effect upon adjacent triangles. Consequently the adjustment of the errors was imperfect, and in computing the interval between any two distant points the result would vary somewhat with the triangles used in the computation—that is, if one computation was made through a chain of triangles running around on the right-hand side, another through a chain of triangles running straight between the two points, and a third through a chain of triangles running around on the left-hand side, the results would usually all differ. At that time things were less highly specialised than now, and all geodetic operations were yet in the hands of first-rate astronomers who soon devised processes for overcoming the difficulty. They imagined every observed angle to be subject to a small correction, and as these corrections were all entangled with each other through the geometrical conditions of the net, by a most ingenious application of the method of least squares they determined them all simultaneously in such a way as to satisfy the whole of the geometrical conditions. Thus the best possible adjustment was obtained, and no matter what triangles were used in passing from one point to another, the result was always the same. That method is now applied to every important triangulation, and its omission would be regarded as proof of incompetency on the part of those in charge of the work.

Now let us compare the conditions existing respectively in a triangulation net and in the group of quantities for the determination of the solar parallax. In the net every angle is subject to a small correction, and the whole system of corrections must be so determined as to make the sum of their weighted squares a minimum, and at the same time satisfy all the geometrical conditions of the net. Like the triangles, the quantities composing the group from which the solar parallax must be determined are all subject to error, and therefore we must regard each of them as requiring a small correction, and

all these corrections must be so determined as to make the sum of their weighted squares a minimum, and at the same time satisfy every one of the equations expressing the relations between the various components of the group.

Thus it appears that the method required for adjusting the solar parallax and its related constants is in all respects the same as that which has so long been used for adjusting systems of triangulation; and as the latter method was invented by astronomers, it is natural to inquire why they have not applied it to the fundamental problem of their own science? The reasons are various, but they may all be classed under two heads. First, an inveterate habit of over-estimating the accuracy of our own work as compared with that of others; and second, the unfortunate effect of too much specialisation.

The prevailing opinion certainly is that great advances have recently been made in astronomy, and so they have in the fields of spectrum analysis and in the measurements of minute quantities of radiant heat; but the solution of the vast majority of astronomical problems depends upon the exact measurement of angles, and in that little or no progress has been made. Bradley, with his zenith sector, a hundred and fifty years ago, and Bessel and Struve, with their circles and transit instruments, seventy years ago, made observations not sensibly inferior to those of the present day, and indeed it would have been surprising if they had not done so. The essentials for accurately determining star places are a skilled observer, a clock, and a transit circle, the latter consisting of a telescope, a divided circle, and four micrometer microscopes. Surely no one will claim that we have to-day any more skilful observers than were Bessel, Bradley, and Struve, and the only way in which we have improved upon the telescopes made by Dollond one hundred and thirty years ago, is by increasing their aperture and relatively diminishing their focal distance. The most famous dividing engine now in existence was made by the elder Repsold seventy-five years ago; but as the errors of divided circles and their micrometer microscopes are always carefully determined, the accuracy of the measured angles is quite independent of any small improvement in the accuracy of the division or of the micrometer screws. Only in the matter of clocks has there been some advance, and even that is not very great. On the whole, the star places of to-day are a little better than those of seventy-five years ago, but even yet there is great room for improvement. One of the commonest applications of these star places is to the determination of latitude, but it is very doubtful if there is any point on the face of the earth whose latitude is known certainly within one-tenth of a second.

Looking at the question from another point of view, it is notorious that the contact observations of the transits of Venus in 1761 and 1769 were so discordant that from the same observations Encke and E. J. Stone got respectively for the solar parallax 8·59 seconds and 8·91 seconds. In 1870 no one thought it possible that there could be any such difficulty with the contact observations of then approaching transits of 1874 and 1882, but now we have found from sad experience that our vaunted modern instruments gave very little better results for the last pair of transits than our predecessors obtained with much cruder appliances in 1761 and 1769.

The theory of probability and uniform experience alike show that the limit of accuracy attainable with any instrument is soon reached; and yet we all know the fascination which continually lures us on in our efforts to get better results out of the familiar telescopes and circles which have constituted the standard equipment of observatories for nearly a century. Possibly these instruments may be capable of indicating somewhat smaller quantities than we have hitherto succeeded in measuring with them; but their limit cannot be far off, because they already show the disturbing effects of slight inequalities of temperature and other uncontrollable causes. So far as these effects are accidental, they eliminate themselves from every long series of observations, but there always remains a residuum of constant error, perhaps quite unsuspected, which gives us no end of trouble. Encke's value of the solar parallax affords a fine illustration of this. From the transits of Venus in 1761 and 1769 he found 8·58 seconds in 1824, which he subsequently corrected to 8·57 seconds, and for thirty years that value was universally accepted. The first objection to it came from Hansen in 1854, a second followed from Le Verrier in 1858, both based upon facts connected with the lunar theory, and eventually it became evident that Encke's parallax was about one quarter of a second too small. Now please observe that Encke's value was obtained trigonometrically, and its inaccuracy was never suspected until it was revealed by gravitational methods which were themselves in error about one-tenth of a second, and required subsequent correction in other ways. Here then was a lesson to astronomers, who are all more or less specialists, but it merely enforced the perfectly well-known principle that the constant errors of any one method are accidental errors with respect to all other methods, and therefore the readiest way of eliminating them is by combining the results from as many different methods as possible. However, the abler the specialist the more certain he is to be blind to all methods but his own, and astronomers have profited so little by the Encke-Hansen-Le Verrier incident of thirty-five years ago that to-day they are mostly divided into two great parties, one of whom holds that the parallax can be best determined from a combination of the constant of aberration with the velocity of light, and the other believes only in the results of heliometer measurements upon asteroids. By all means continue the heliometer measurements, and do everything possible to clear up the mystery which now surrounds the constant of aberration; but why ignore the work of predecessors who were quite as able as ourselves? If it were desired to determine some one angle of a triangulation net with special exactness, what would be thought of a man who attempted to do so by repeated measurements of the angle in question, while he persistently neglected to adjust the net? And yet, until recently, astronomers have been doing precisely that kind of thing with the solar parallax. I do not think there is any exaggeration in saying that the trustworthy observations now on record for the determination of the numerous quantities which are functions of the parallax could not be duplicated by the most industrious astronomer working continuously for a thousand years. How then can we suppose that the result properly deducible from them can be materially affected by anything that any of us can do in a lifetime, unless we are fortunate enough to invent methods of measurement vastly superior to any hitherto imagined? Probably the existing observations for the determination of most of these quantities are as exact as any that can ever be made with our present instruments, and if they were freed from constant errors they would certainly give results very near the truth. To that end we have only to form a system of simultaneous equations between all the observed quantities, and then deduce the most probable values of these quantities by the method of least squares. Perhaps some of you may think that the value so obtained for the solar parallax would depend largely upon the relative weights assigned to the various quantities, but such is not the case. With almost any possible system of weights the solar parallax will come out very nearly 8·809 seconds $\pm$ 0·0057 seconds, whence we have for the mean distance between the earth and sun 92,797,000 miles, with a probable error of only 59,700 miles; and for the diameter of the solar system, measured to its outermost member, the Planet Neptune, 5,578,400,000 miles.

THE METEOR AND METEOR-STREAK OF AUGUST 26, 1894.

THE present year will certainly be remarkable for its large meteors. One of the most brilliant class of these phenomena appeared on January 25, and a fortnight later (February 8) a fireball was seen as a conspicuous object even in the presence of the midday sun, for the time was only 28 minutes after noon. The early evening of February 21 furnished another of these brilliant objects, but the observations were neither numerous nor exact, and all that could be definitely gleaned from them was that the body disappeared at a height of 30 miles over Bolton in Lancashire. On April 22, before daylight had gone, a fine meteor descended over the extreme south-east part of England, crossing the Strait of Dover from Hastings in the direction of Amiens in France. On May 18 a large daylight meteor was observed in Scotland and Ireland. Several additional instances of these striking visitors have been recently recorded, and the Perseids presented a few fine specimens, though the season has been a very cloudy and unpropitious one for all kinds of celestial observation.

The magnificent meteor which forms the subject of this paper, appeared on August 26 at 10h. 20m. It did not owe its parentage to the great Perseid system, for it came too late in the month, and, moreover, its direction of flight is not conformable.

Those fortunate persons who happened to be out of doors at the time named, were startled by a bright lightning-like flash,[1] and naturally looking upwards for the cause, they either saw the end part of a fine meteor, or the dense streak it had projected as a glowing column of phosphorescence upon the dark ground of the sky. This streak was quite a remarkable feature in connection with the meteor, for on three grounds it merits careful consideration—viz. for its duration, for the proper motion it soon exhibited under the influence of the atmospheric current in which it was situated, and for the nondescript shapes it assumed. Before giving any particulars it may, however, be interesting to quote from some of the descriptions.

Mr. H. Corder, Bridgwater, writes that on August 26, 10.20, there was a very interesting meteor, but that its actual descent was unfortunately hidden by a wall. He afterwards, however, saw a bright streak in the position $138° + 62°$ to $137\frac{1}{2}° + 58\frac{1}{2}°$. This, as seen with a binocular, soon became crooked, and drifted very slowly, until it finally disappeared at 10.50, half an hour afterwards, at the point $98° + 64°$. Mr. Corder adds that this is the longest duration of any meteor-streak he has ever seen.[2]

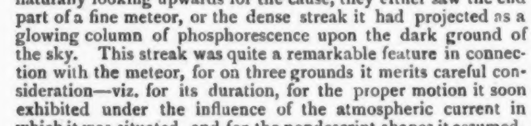


FIG. 1.—Successive appearances of the meteor-streak as observed by Mr. Corder at Bridgwater.

Mr. S. A. Saunder, Crowthorne, near Wokingham, reports the time as 10.19. The length of the meteor's path, as he observed it, was about 20°; it moved quickly, and left a persistent streak of about 3° in length some 5° above its point of disappearance. This remained visible as a distinct trail for 20 or 30 seconds, and as a decreasing luminous patch at about $185° + 44°$ for two or three minutes. The path must have slightly preceded ϵ Ursæ Majoris, and probably crossed $190° + 57°$

A correspondent, writing to the *Daily News* from Wood Green, says that at 10.18 he observed an exceptionally brilliant meteor about the size of a cricket-ball and of a pale blue colour. It appeared near to the foremost star in Ursa Major, and was visible for some seconds. The chief peculiarity in addition to its great brilliancy, was that it left a long broad trail of light in the sky, which remained some time after the meteor had vanished, and then faded away very slowly.

Mr. E. W. Coker, writing from Coventry to the *English Mechanic*, states that at 10.20 he observed a nebulous light in the north-west part of the heavens. Its form was elongated, and he compares it with a dense cluster of star-dust. He saw it first through a window with the naked eye, and brought a telescope to bear upon it, but it had evidently passed its brightest, and was then fading rapidly. Its situation was in $213° + 48°$.

Colonel G. L. Tupman has kindly sent me two observations from Harrow, which mutually corroborate each other as to the end point near α Boötis (Arcturus) in azimuth 110° west of south, and altitude 11° or 12°. The streak endured so long that one of the observers got a telescope and watched it for some time. He remarked that the lower part of the streak and Arcturus would have been in the field of his finder together.

Mr. T. M. Dunmur, writing from Trefriw, says:—"The meteor, while excelling in brightness though not in duration of flight any that I have had the good fortune to witness, was in respect of the glowing trail which succeeded quite unprecedented in my experience. It was vividly luminous, and the denser portion at once began to collect in a semicircular shape. This breaking-up absorbed the remainder of the trail, and still clearly visible slowly drifted toward, across, and beyond the 'Milky Way,' when it faded from sight not less than eight minutes from its first appearance."

There are some other descriptions from the Midlands and South of England, but it is singular that no reports have come to hand from North Wales, Cheshire, and Lancashire, over which the region the meteor appeared, and where its brilliancy must certainly have been very great.

[1] The flash was seen by the writer at Bristol while comet-seeking, but the meteor itself and its streak were hidden by a house which obstructs the view of the north-north-west sky.

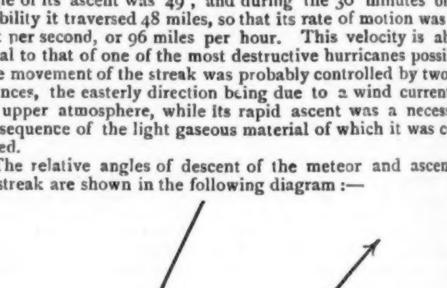
[2] The most durable streak ever seen by the writer was that of a Leonid fireball which appeared on November 13, 1866 at 12.30, and left a visible streak for three-quarters of an hour.

On comparing the various observations together, it is found that they agree much better than is usually the case in a miscellaneous collection of this sort. The real path of the meteor in the air is therefore determinable within very moderate limits of error. The Harrow position near Arcturus for the end part of the streak, offers, however, a discordance with the other observations, and there is reason to suppose that the star α Canum Venaticûm was mistaken for Arcturus. It is necessary for consistency that the azimuth of 110 west of south, as seen from Harrow, should be increased to 132° west of south, when it will be correctly directed towards Denbigh, near which place the meteor streak was situated when first evolved.

When the meteor was first seen it was about 90 miles high and over the river Mersey, at a point 20 miles west of Ormskirk. Passing rapidly almost due south, it ended at a height of 30 miles above Ruthin, Denbighshire. The angle of the meteor's descent was 63°, and the length of its observed path 66 miles. The earth-point is indicated 6 miles south of Llangollen, and the astronomical radiant was at $305° + 79°$ near the $4\frac{1}{2}$ mag. star κ Cephei. This position is confirmed by a statement of Mr. Corder's, that he saw a few other meteors giving a radiant at $300° + 80°$, and believed the large meteor would be found to belong to it, though its direction was not precisely conformable.

The luminous streak was about 9 miles in length, and its central portion 47 miles high over a place 6 miles east-north-east of Denbigh. The direction of its drift was eastwards towards Chester, and it passed over that town at a height of 68 miles, so that it was ascending rapidly in the atmosphere. It disappeared, according to the last view obtained of it by Mr. Corder, at 10.50 with his binoculars, when vertically over a point 2 miles west of Middlewich at a height of 83 miles. The angle of its ascent was 49°, and during the 30 minutes of its visibility it traversed 48 miles, so that its rate of motion was 141 feet per second, or 96 miles per hour. This velocity is about equal to that of one of the most destructive hurricanes possible. The movement of the streak was probably controlled by two influences, the easterly direction being due to a wind current in the upper atmosphere, while its rapid ascent was a necessary consequence of the light gaseous material of which it was composed.

The relative angles of descent of the meteor and ascent of its streak are shown in the following diagram:—



At the time of the meteor's appearance the air appears to have been pretty calm; in the north of England the wind was very slight from east, while in the south the direction was from south. The surface current would therefore appear to have been very different to that at a great altitude.

The radiant of the fireball is not a well-known one for the date, but in 1893 I saw a few meteors during the period from August 4–16 from $310° + 77°$, and in 1885, September 4–5, a feeble radiant was seen at $315° + 76°$. On September 1, 1878, at 10.20, I observed a very brilliant streak-leaving meteor with a path from $161° + 70°$ to $155° + 56°$, and attributed the radiant as at $315° + 76°$. On September 8, 1878, and on September 7, 1888, I saw fireballs, brighter than Venus, that were directed from the same radiant point. There would appear, therefore, to be a well-defined shower of large meteors from the northern part of Cepheus at the close of August and beginning of September. Mr. Corder also informs me that on September 8, at 11.3, he saw another fireball descending from $142° + 54°$ to $144° + 51°$ in Ursa Major. The same object was seen at Leeds, from which place it was projected on the stars of Aquarius, and there is reason to believe that this brilliant meteor, like that of August 26, and the fireballs of September 1878 and 1888, before referred to, had their derivation from the shower of κ Cepheids.

W. F. DENNING.

THE INDEXING OF CHEMICAL LITERATURE

AT the recent meeting of the American Association for the Advancement of Science the Committee on Indexing Chemical Literature presented to the Chemical Section its twelfth annual report. The following is a reprint of an advance copy of the report. During the current year the following bibliographies have been printed in the channels indicated:—

(1) Index to the Literature of Didymium, 1842-1893. By A. C. Langmuir. School of Mines Quarterly (Columbia College, New York). Vol. xv. pp. 33-47. November 1893. In this index the author follows the plan originally proposed by H. C. Bolton in 1870.

(2) The Tannins, a monograph on the history, preparation, properties, methods of estimation and uses of the vegetable astringents. With an index to the literature of the subject. Vol. ii., the Tannins of oak-bark, mangrove, canaigre, chestnut. By Henry Trimble. Philadelphia, 1894. Pp. 172. 12mo. Ill.

This forms the second volume of the work previously noted in our reports. The carefully compiled bibliography contains about 325 titles.

Reports of progress have been received from several chemists. Prof. Arthur M. Comey announces that the first volume of his Dictionary of Chemical Solubilities, devoted to inorganic compounds, has gone to press, and will be published before the close of the year. The second volume is also in active preparation.

Dr. Alfred Tuckerman reports that the United States Section of his Bibliography of Mineral Waters will be ready for the printer in a few months.

Prof. Clement W. Andrews states that he had done much work on a Bibliography of the Polariscopic Determination of Sugar; but, learning that Prof. H. W. Wiley, chief chemist of the U. S. Department of Agriculture, was engaged in a similar undertaking, generously handed over to him all the material he had accumulated. The combined manuscripts have recently been returned to Prof. Andrews, who will continue the work.

Prof. H. W. Wiley reports great activity on the part of the Division of Chemistry of the U. S. Department of Agriculture, in the preparation of bibliographies and special indexes, but he is obliged to admit difficulties in securing the printing of the manuscripts. We quote the following paragraphs from his letter, dated June 29, 1894, addressed to the chairman of the committee:—"The elegant bibliography of heavy metals occurring in canned goods, by Mrs. K. P. McElroy, has not yet found an avenue for publication." . . . "We also have a very complete bibliography of carbohydrates from the point at which they were left by Tollens in his Handbuch, in 1888, up to the close of 1892. This work was partly done by myself, but chiefly by Mr. H. E. L. Horton, and we were assisted greatly by receiving many hundred titles from Prof. C. W. Andrews, of the Massachusetts Institute of Technology. But what we can do with such a bibliography, comprising as it does three or four thousand titles, I do not know. The Department of Agriculture will not publish it, it is too large for the Journal of the American Chemical Society, and so it lies idle." . . . "A very complete bibliography of agricultural chemistry for the year ending 1893 has also been completed by the committee appointed by the Association of the Official Agricultural Chemists, of which Dr. William Frear is chairman. This bibliography I submitted to the Assistant Secretary of Agriculture with the request that it be published as a part of the Proceedings of the Association, but this request was not complied with. The same committee has in preparation a complete bibliography of agricultural chemistry for the year ending June 30, 1894, and this report will be presented to the meeting of the Association of Official Agricultural Chemists in August at Washington. We shall then have unpublished a complete bibliography of all agricultural chemical topics for the two years ending June 30, 1894."

Mr. P. H. Seymour's "Bibliography of Aceto-Acetic Ester" is in the printer's hands, and will be published by the Smithsonian Institution during the summer.

Prof. F. W. Clarke reports that he is engaged on a new edition of his "Recalculation of Atomic Weights."

Prof. H. C. Bolton has begun a "Supplement to his Bibliography of Chemistry," and last winter visited the chief libraries of Italy in search of material.

Dr. H. P. Talbot, of the Massachusetts Institute of Technology, with the co-operation of Dr. H. C. Bolton, has begun a second edition of the "Index to the Literature of Manganese," published by the latter in 1875, with the intention of bringing it down to date.

Prof. James Lewis Howe, of Louisville, Ky., reports progress on a Bibliography of the Platinum Metals.

Dr. W. H. Magee, of Cornell University, has completed Indexes to the Literature of Cerium and of Lanthanum, and the MSS. have been approved by your committee, and, together with Mr. Langmuir's Index to the Literature of Didymium, have been recommended to the Smithsonian Institution for publication. The three Indexes have been accepted by the Smithsonian, and will appear in the Miscellaneous Collections.

Prof. Charles E. Munroe reports that part ii. of his "Index to the Literature of Explosives" does not complete his work, as stated in the Eleventh Annual Report; he is engaged on a continuation.

Dr. Claude Augustus Oscar Rosell's thesis, presented to the Columbian University, Washington, D. C., in June, entitled "Investigation of the Properties of Ferric Acid," contains an exhaustive bibliography of the Ferrates and Ferric Acid; the channel of publication is not yet determined.

Prof. J. Christian Bay reports progress on a bibliography of alcoholic fermentation, and has commenced a bibliography of glycogen.

The annual reports of this committee are properly confined to the productions of Americans; but the chairman begs leave to direct attention to indications of a growing appreciation of the value of special bibliographies on the part of European chemists, confirming by their recent and proposed activities the work begun in America, at the chairman's suggestion, now more than twelve years ago. Several European countries have long published periodical bulletins of all books issued in their own lands, but they are, as a rule, too comprehensive in scope for the convenience of the specialist in science. Since the "Biblioteca Historico Naturalis," published at Göttingen, dropped chemistry from its pages (in 1887) the most useful bibliography of current scientific works has been the well-known "Naturæ Novitates" (Friedlaender, Berlin), now in its sixteenth year; however, this trade serial is stronger in German than in other languages, and falls short of the completeness desirable.

In technology and technical chemistry the admirable "Repertorium der technischen Literatur" (Leipzig), in its continuation, affords invaluable assistance to the industrial chemist. Recently, too, the following periodical has been established: "Biblioteca polytechnica; internationale Bibliographie der gesammten neuen technischen Literatur, herausgegeben von Fritz von Szczepanski." (St. Petersburg and Leipzig, 1893.) 8vo. 12 numbers per annum. This includes chemistry pure and applied.

The need of an exhaustive authoritative bibliography of current chemical books of the world is still felt.

In a private letter to the chairman of your committee, Dr. Bechhold, of Frankfort-on-Maine, announces his intention of publishing a full and complete Index to Current Chemical Literature in all languages, on a most comprehensive plan; the first number of this serial will be awaited by chemists with great interest.

Heinrich Wien (Vienna) and F. A. Brockhaus (Leipzig), announce the publication of a "Universal Index to the World's Technical and Scientific Literature." This ambitious undertaking is intended to embrace both books and periodicals, and to represent all the known literature that has appeared in every part of the world; five parts are projected, viz. chemistry, medicine, mining, photography, electricity.

As most of the members of the Chemical Section are aware, a call has been issued for an International Congress of Applied Chemistry, to be held, under the patronage of the Belgian Government, at Brussels in August 1894. At that meeting it is proposed to found a Review of Reviews of Applied Biological Chemistry in several languages, to contain a *résumé* of chemical work in that branch from all parts of the world. The Secretary General of the Congress is M. Sachs, 168 Rue d'Allemagne, Brussels.

At the Congress of Chemists held in Chicago, in August 1893, your chairman had the honour to read an address on an "International Index to Chemical Literature" (*J. Am. Chem. Soc.*, xv. Oct. 1893), in which he proposed a simple scheme

for indexing current periodical chemical publications by international co-operation. It is extremely gratifying to record that the necessity of international co-operation has since been suggested by so weighty an authority as the Royal Society (London). That splendid monument of the bibliography of science, "The Catalogue of Scientific Papers," published by the Royal Society, has failed to satisfy the requirements of students in science, owing to the lack of a subject-index to the prodigious material classified under the names of the authors; but according to a circular issued by the Royal Society in April, "it is hoped that a key to the volumes already published may be eventually issued." The Royal Society further announces its intention of continuing the "Catalogue of Scientific Papers" after Jan. 1, 1900, on an enlarged and improved plan, with the aid of international co-operation, and asks for suggestions as to the best methods of inaugurating such a scheme.

Although this report deals with chemistry, it may be proper to mention here an important undertaking in another branch of science, as it affords an additional instance of the progress now making towards international co-operation in bibliography. At the Washington meeting of the International Congress of Geologists a committee on the Bibliography of Geology was appointed for the purpose of preparing a list of the geologic bibliographies now in existence. This work is now approaching completion under the direction of M. Emmanuel de Margerie (Paris), the Secretary of the International Committee.

American botanists also are showing their appreciation of bibliographical work. A committee of the Torrey Botanical Club publishes in the Bulletin of the Club an "Index to recent Literature relating to American Botany." This Index was begun in January 1894, and is continued each month; the arrangement is alphabetically by authors.

At the Chicago Congress of Chemists a committee was appointed to bring about the organisation of a triennial (or quinquennial) international meeting of chemists. Prof. Frank W. Clarke, one of the members of your committee, is chairman of that body. Perhaps the future World's Chemical Congresses may arrange the publication of an exhaustive Index to the Chemical Literature of the World by international co-operation, either in accordance with the scheme proposed by your chairman in his address at Chicago, or in some more efficient way.

Thus, it is evident that immense progress is being made in the compilation of indexes and bibliographies in many branches of science, in both Europe and America; it is to be hoped that American chemists, who have been in some measure pioneers in the matter, will feel stimulated to still greater exertions than before.

The chairman has a limited number of copies of the Tenth Report, containing a list of forty-five Indexes to Chemical Literature, which he will be glad to send to applicants. Communications should be addressed to the Chairman, at the University Club, New York City.

Committee,
- H. CARRINGTON BOLTON, *Chairman*,
- F. W. CLARKE,
- ALBERT R. LEEDS (in Europe),
- ALEXIS A. JULIEN,
- JOHN W. LANGLEY,
- ALBERT B. PRESCOTT (in Europe),
- ALFRED TUCKERMAN.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 17.—M. Lœwy in the chair.—A note was presented by M. Faye concerning the International Geodetic Association at Innspruck, and intimating the probability of a certain number of geologists being requested to join its permanent Commission.—Shooting-stars observed in Italy in August 1894; a note by P. François Denza. The numbers of shooting-stars observed at some four-and-twenty stations scattered all over Italy are recorded. The swarm was thicker on the night of 10–11 than on other nights. By observations made at the Vatican, the principal radiant point had the co-ordinates $\alpha = 45°$, $\delta = 54°$.—On the problems of dynamics of which the differential equations allow an infinitesimal transformation, by M. P. Stäckel. An infinitesimal transformation P_f, which allows $n - 1$ differential equations between the independent variables $p_1, p_2, \ldots, p_n$ which determine the position of the mobile system, does not exist when the variables $p_1, p_2, \ldots, p_n$ are so chosen that: (1) The function of the forces Π depends only on $p_2, p_3, \ldots, p_n$; (2) the expression of the acting force is reduced to

$$\tfrac{1}{2}e^{cp_1}\sum_{k,\lambda} b_{k\lambda}(p_2, p_3, \ldots, p_n)\frac{dp_k}{dt}\frac{dp_\lambda}{dt};$$

c is an arbitrary constant, and the coefficients $b_{k\lambda}$ depend only on the arguments $p_2, p_3, \ldots, p_n$. Then the infinitesimal transformation P_f has the canonic form $P_f = \frac{\partial f}{\partial p_1}$. These conditions are necessary and sufficient.—On the linear equations from the derived partials of the second order, by M. A. Petot.—On the mixture of liquids, by M. J. de Kowalski. The author has endeavoured to obtain experimental confirmation of Van der Waals' theory of the miscibility of liquids if sufficient pressures be applied. Negative results only were obtained in the cases of isobutyl alcohol and water and ether and water. The system ethyl alcohol: isobutyl alcohol: water with a blue colouring matter, completely mixing at 22°·7, gave at 19°·5 mixture at a pressure of 880 to 900 atmospheres. At 19° the same system showed no signs of becoming homogeneous, even under a pressure much greater than 1000 atmospheres.—On the presence of *Thylles gommeuses* in the vine, by M. Louis Mangin.—On a vine disease caused by *Aureobasidium vitis*, by M. P. Eloste. The disease known as the "maladie rouge" has been widely disseminated this year. The author has found the mycelium of *Aureobasidium vitis* in the altered parts of the leaves, but he has not yet found its fructifications, nor is its parasitism completely proved; a full description of the progress of the disease in an attacked plant is given.—A waterspout at sea, by M. Génot.

BOOKS RECEIVED.

BOOKS.—Fertilisers and Feeding-Stuffs: Dr. B. Dyer (C. Lockwood).—Proceedings of the Royal Physical Society, Session 1893–94 (Edinburgh).—University College, Bristol; Calendar for the Session 1894-95 (Bristol, Arrowsmith).—Newfoundland as it is in 1894: Rev. M. Harvey (K. Paul).—Lehrbuch der Bakteriologischen untersuchung und Diagnostik: Dr. L. Heim (Stuttgart, Enke).—The Collected Mathematical Papers of Arthur Cayley, Sc.D., F.R.S., Vol. vii. (Cambridge University Press).

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